Popular Astronomy.

Vol. II. No. 2. OCTOBER, 1894. Whole No. 12.

THE LARGE SUNSPOT OF AUGUST, 1894.

H. C. WILSON.

The most prominent sunspot group which has been visible during this year appeared at the east limb of the Sun on Aug. 10, passed the center Aug. 16, and disappeared by rotation between Aug. 22 and 23. While it did not approach in magnitude the great group of February, 1892, it rivalled that of July, 1893, and exceeded that of February, 1894, mention of which has been made in recent publications.

We have thought it would interest the readers of POPULAR ASTRONOMY to see reproductions of some of the photographs of this spot-group, which were taken at Goodsell Observatory, and to know something about the actual motions of the spots upon the Sun. We aim to get a good photograph of the Sun every day (Sundays excepted), but are prevented by clouds about one third of the time. On another third, probably, of the days the seeing is poor, so that we do not expect to get good sharp photographs on more than one third of the days in the year. The series taken in August was only fairly successful, the sky being unusually hazy and the weather extremely hot. Photographs were obtained on the dates Aug. 10, 11, 14, 15, 16, 17, 18, 20, 21, 22 and 23. From these we have selected the five which are reproduced in Plates VI, VII and VIII, not as the best, but as showing best the progress of the spots across the solar disc. The two enlarged pictures of the spot on the 16th and 17th were taken when the seeing was best and show marked changes in the details of the group in the interval of one day. These enlargements were made directly in the telescope, the diameter of the whole solar image on the same scale being about 7½ inches.

All the photographs were taken with the 8-inch Clark refractor, with a third objective lens, and double concave amplifiers, the latter made by Brashear. The exposures were made by means of a slide, having a narrow slit about $\frac{1}{16}$ of an inch wide, driven through the beam of rays by a spring, giving an exposure of about one thousanth of a second duration. On some of the dates mentioned we were experimenting with a slide propelled by rub-

ber bands. This did not give a uniform exposure to all parts of the disc. The reader will notice the effect in the rapid shading off toward the N. W. edge of the disc. The horizontal black line is the image of a silk fiber stretched just in front of the sensitive plate and adjusted parallel to the celestial equator. This is not parallel to the equator of the Sun and we have therefore drawn another line indicating the position of the Sun's axis, which deviates to the east of north from 15° to 18° during the period under consideration.

On the photograph taken Aug. 10, $12^h\ 07^m$ P. M. central time, the spot appears as a mere notch in the east edge of the Sun's disc. Aug. 11, $12^h\ 21^m$ P. M. two quite prominent spots with bright faculæ closely surrounding them are seen a little way within the east edge (See Plate VI). We will designate the northernmost one of these as A and the other as B. The solar longitude of A is 27°.5 and its latitude + 6°.9 on this date. The longitude of B is 21°.0 and its latitude + 3°.9.

Aug. 14 at $2^h\ 12^m$ P. M., Plate VI, the aspect of the group is very much changed. A long spot with several umbræ has developed at the southern edge of A. The reader will take especial note of this spot, which we will designate C. The east end is now the more prominent. Two considerable spots D and E have appeared to the northeast of B.

Aug. 15, $5^h\ 20^m$ P. M. spots A and B both have nearly round funnel-like umbræ. C has a long serrated umbra. The west end is now the more prominent and there is a small detached spot directly west of it. D and E are waning.

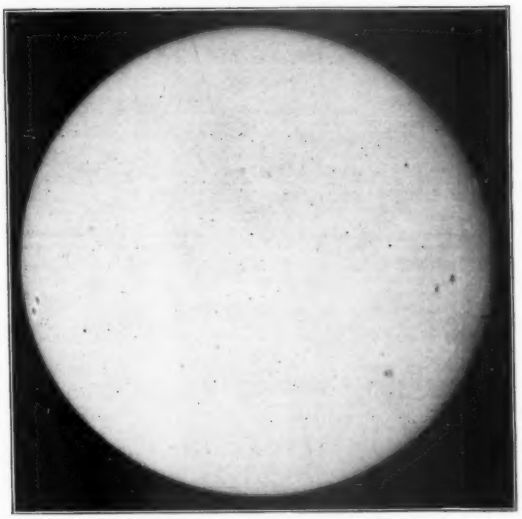
Aug. 16, $5^h\ 00^m$ P. M. A has a decided funnel form, while B has lost that form, the penumbra beginning to enter the south side of the umbra. That portion of C which is directly south of A has nearly closed up, while the western portion has apparently pushed forward and begun to rotate in the direction opposite to the movement of the hands of a clock. The spots D and E are almost lost in the groups of small ones which have developed about them.

Aug. 17, $5^h\ 24^m$ P. M., the spot C has become almost entirely detached from A and its umbra has assumed the shape of a rude rectangle with the longer sides almost due north and south. B has been cut in two by the bridge which was noticed as beginning to form on the 16th.

Aug. 18, $4^h\ 30^m$ P. M., the general appearance of the group is about the same as on the 17th, but there are many changes in the less prominent details. The umbra of C has lost its rectangular

PLATE VI.

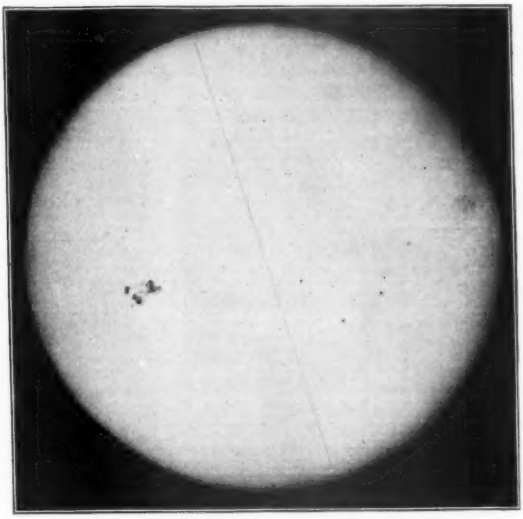
N



S

Aug. 11, 1894, $12^h\ 21^m$ P. M.

N



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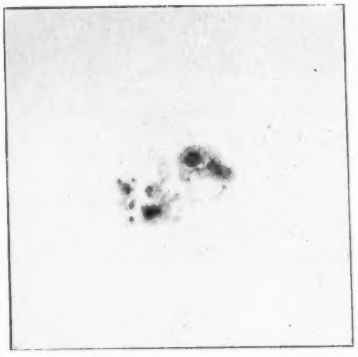
Aug. 14, 1894, $2^h\ 12^m$ P. M.

THE GREAT SUNSPOT GROUP OF AUGUST 1894.

From photographs taken at Goodsell Observatory.

PLATE VII.

N



S

Aug. 16, 1894, $4^h\ 19^m$ P. M.

N

S

Aug. 17, 1894, $4^h\ 57^m$ P. M.

THE GREAT SUNSPOT GROUP OF AUGUST 1894.

From photographs taken at Goodsell Observatory.

shape, and is now oval. There is a long streak, with minute umbræ, running east from C past the lower edge of A, looking as if it were the rent left in the solar surface by the deep-seated force, which produced C, in its westward progress. The eastern half of B has begun to wane. In this photograph there are two conspicuous spots on the upper half of the disc, which are due to flaws in the original negative.

Aug. 20, $12^h\ 08^m$ P. M., the spot C is the most prominent one in the group while B has greatly diminished and many of the smaller ones have entirely disappeared.

Aug. 21, $12^h\ 44^m$ P. M., the appearance of the group is essentially the same as on the 20th, except that it is more foreshortened and that the faculæ show more plainly around it. The area of faculæ is much larger and brighter than that which surrounded the group when it was in a corresponding position near the east edge of the disc Aug. 11.

The following table gives the solar longitudes and latitudes measured for the three principal spots, A, B and C, from which it appears that these spots all advanced in longitude and increased a little in latitude, and that the increase in both was more rapid in the earlier than in the latter half of the period. The great advance of C appears not to have been due to the movement of any one umbra, but the transfer of intensity from one to another in succession, as if some deep-seated source of disturbance were progressing westward, its effect coming to the surface only at intervals. It may have been due, on the other hand, to a force above the surface, alternately rising and lowering as our cyclones or tornadoes sometimes do.

TABLE SHOWING THE LONGITUDES AND LATITUDES OF THE THREE PRINCIPAL SPOTS OF THE AUGUST, 1894, GROUP.

Central Time.			A		B		C	
	h	m	Longitude. °	Latitude. °	Longitude. °	Latitude. °	Longitude. °	Latitude. °
Aug. 10	12	07	24.7	+ 6.4				
11	12	21	27.5	+ 6.9	21.0	+ 3.9		
14	12	26	28.1	+ 7.9	23.4	+ 4.3	28.2	+ 6.9
							29.9	+ 6.3
15	5	20	28.5	+ 8.4	23.4	+ 4.6	27.4	+ 6.9
							30.3	+ 6.6
							31.0	+ 6.2
16	5	00	28.9	+ 8.3	23.0	+ 4.6	27.5	+ 6.2
							30.1	+ 7.1
							31.0	+ 6.2
							31.6	+ 5.8
17	5	24	28.6	+ 8.4	23.6	+ 4.6	31.3	+ 6.2
					22.3	+ 4.5		
18	4	30	29.1	+ 8.6	23.7	+ 4.8	32.0	+ 6.1
					22.6	+ 4.5		
20	12	08	29.4	+ 8.6	24.7	+ 5.4	32.3	+ 6.7
21	12	44	29.8	+ 8.6	25.3	+ 6.1	31.9	+ 7.0

The dimensions of the group can easily be measured from the photographs. It appears to have reached its maximum development about Aug. 15, when its length was one tenth of the Sun's diameter or 86,000 miles and its width about 40,000 miles. The black centers of spots A and B then, and of C later, were each large enough to contain the Earth, with room to spare.

The group reappeared on the east limb of the Sun Sept. 5. On Sept. 7 the photograph shows it greatly diminished in size. Only two inconspicuous spots are seen, the larger in longitude 34°.8, latitude + 9°.4 and the smaller in longitude 37°.4, latitude + 5°.8. We take these to be the remnants of A and C respectively. A large single spot, which, before the measures were made, we took for the remains of the August group has appeared in longitude 37°, latitude + 18°. This spot is quite regular in outline and has a very black umbra.

The remnants of spots A and C faded out rapidly, C being invisible on Sept. 10 and A reduced to the merest speck on the photograph taken Sept. 15.

In attempting to trace the history of the group backward we find that in July there was no group near enough to the same longitude to be identified with the August group, although there was a quite large spot in longitude 52°, latitude + 8°, and a group of four large spots in longitude 345° — 359°, latitude + 13°.

MARS.

PERCIVAL LOWELL.

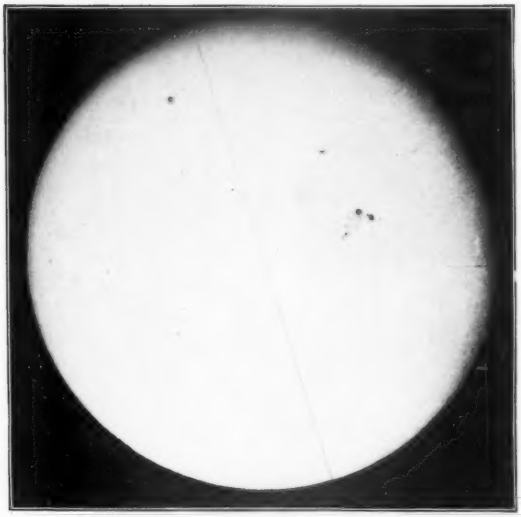
The Polar Snows.

On "the snowy poles of moonless Mars" the snow remains though the moonlessness has departed. Indeed, difficult as it is to be sure of anything across that great gulf of space that parts us from our closest of cosmic kin we are nevertheless more certain of Mars' polar snows than of any other physical fact about him.

Conspicuous above every other feature of the planet's disk are two white circular spots covering what rotation identifies as the planet's poles. The two are not, however, commonly in view together, the tilt of the planet's axis hiding the one as it displays more completely the other. At present the south pole is bowed toward us, at an angle of about 17°.

PLATE VIII.

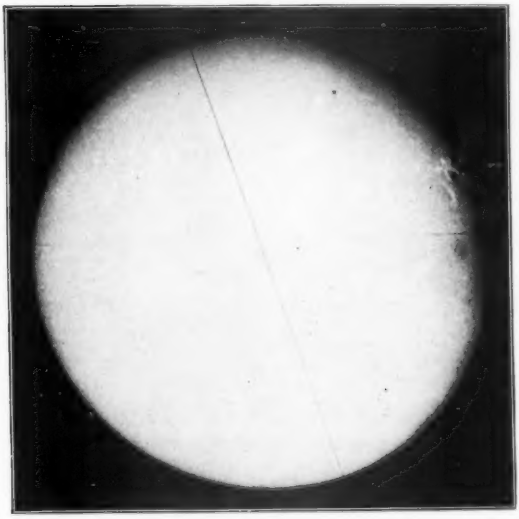
N



S

Aug. 18, 1894. $4^h\ 30^m$ P. M.

N



S

Aug. 21, 1894, $12^h\ 44^m$ P. M.

THE GREAT SUNSPOT GROUP OF AUGUST 1894.

From photographs taken at Goodsell Observatory.

To Huyghens, to whom we owe the sketch of the first recognizable marking on Mars, we owe also the first picturing of these caps. In a drawing of his made at ten o'clock on August 13th, 1672, the south polar cap appears unmistakably.

To Maraldi we are indebted for the first study of them. During the opposition of 1719 he observed that the south polar patch, the one then in view, decreased in visibility as time went on, but with apparent fluctuations. During one-half of each presentation* it suffered but slight diminution, while for the other half it grew steadily less conspicuous till finally it vanished. Then in later presentations it as gradually reappeared. From which he argued that its changes were connected with the rotation of the planet, yet in such a manner as to imply physical changes in the spot itself. He, however, likened it to the spots on the Sun and to the cloud-belts of Jupiter.

In 1777 Sir William Herschel began a systematic observation of Mars with special reference to his polar spots and his rotation which he continued for four successive oppositions and the outcome of which was his announcement that these white caps were polar snows, an explanation of them which has received only confirmation since. He based his belief upon the fact that the size of the spots followed in due course upon the Martian seasons. In the springtime of its year each showed as a huge round spot; from which importance it slowly dwindled as summer came on till by autumn it had decreased to an insignificant patch, which then began to grow again and by late winter had attained to its former *proportions*.

Sir William Herschel also discovered that the south polar patch did not lie exactly over the pole of the planet; that is, that the pole of cold and the geographical pole did not coincide.

Beer and Mädler were the next to extend our knowledge of these far-off snows. They first measured them and their eccentricity; measures which others including especially Schiaparelli, have since carried to greater perfection. At its maximum the south polar cap is about 70° across, the north polar 60°, and each decreases to a minimum of 4 or 5 degrees. The centre of the south polar cap lies about 5 degrees from the south pole in lat. 30°; the north polar one, however, is found to be concentric with the pole. This eccentricity of the southern pole is indeed so

* As Mars rotates in about $24^h\ 37^m$ and the Earth in about $23^h\ 56^m$, this combined with the revolutions of both planets round the Sun makes on the average, 36 rotations of Mars nearly equal to 37 rotations of the Earth. Once, therefore in every 37 of our days Mars shows us again the same face at the same hour.

marked that it may easily be detected at this present moment by a very small instrument. If the observer will map the position of the true pole on a disk prepared from Marth's data and then look at the planet he will at once perceive the eccentric position of the little glistening patch which is all that is now left of the superb snow-stretch of two months ago.

The size of these winter's night-caps of Mars seems to us, at first, huge; but let us try to see ourselves as others see us, if others there be to speculate on the subject. In the depths of winter our own north polar snow-cap stretches one unbrokeu white sheet from the pole to about lat. 45°, in some parts of the northern hemisphere. That is to say it is in some directions 90° across. To a Martian we must appear to be pretty well snowed under. Indeed for a time we live in the midst of the snow-cap, a pregnant thought toward our conception of life in other worlds.

Curiously enough—unless indeed there be a necessary though as yet unexplained connection between the two—the Martian south pole is apparently the most surrounded by water just as the Earth's is, while the winter of the southern hemispheres of both planets takes place when the planets are near aphelion. The latter cause would accentuate the seasons, the former temper them. In the case of Mars the two antagonistic forces would seem practically to balance one another—as the snow accumulates about equally in both hemispheres and melts to about the same minimum in both.

The subsequent history of our knowledge of these snow caps of Mars should be of some interest in view of the extraordinary amount of enthusiasm expeditions to our own north pole excite. For at much less expense and at absolutely no hazard, astronomy has quietly conducted polar expeditions to Mars so successfully that we now know more about the Martian south polar regions than we do about either of our own.

Beer and Mädler first observed that a dark band surrounded the north polar spot and continuously. The same has been noticed by Schiaparelli. Oddly enough nobody seems hitherto to have specially remarked that the south polar spot is similarly girdled. The phenomenon, however, is very marked and the fact that in both the band keeps pace with the spot's contraction, is highly significant. For it amounts to proof that these belts are polar seas, quite large in mid-spring and becoming smaller with the spot itself. How this water disappears is not quite certain; whether it be caught up into heaven and thence descends

upon other parts of the planet's surface which some infer, or whether it runs through long straits to the equator of which we have apparent evidence, certain it is that it leaves the pole only to reappear there the next season.

Though tolerably uniform in width for the greater part of its length, the sea expands in places into broad bays.

But besides this ring of antarctic ocean, there turns out to be at the south pole at the proper season, that dream of arctic explorers, an open polar sea. It lies in a valley between two mountain ranges. Of this we are almost as sure as if we had climbed one of the enclosing summits and looked down upon it.

In 1892 as the snow began to decrease, a long dark line was observed at Arequipa, Peru, to make its appearance in the midst of the cap and then gradually to grow till it cut the cap in two. At the corresponding Martian season this year the same dark line was observed at Flagstaff in the same position. It was well placed for observation and was carefully studied. It started apparently not far from the centre of the snow-cap and increased in width and length till it opened into the polar sea in longitude 160°. Later its opposite end came out in longitude 330°. Meanwhile it was spreading even faster in the middle. While it was thus eating into the snow, some brilliant points, shining like stars, were observed in the snow between it and the outer edge of the cap—on several successive mornings. They undoubtedly were the far off glisten of snow-slopes. The subsequent behaviour of the spots from which they came bore witness to this. For the rift grew till it formed a large lake in the midst of the snow not far from the geographical pole. Other rifts also appeared and ate into the cap but the spots that had shone out so brilliantly still remained as snow islands, though daily diminished in size. At last the smaller of the two chief portions into which the cap had been split dwindled entirely away and the other became a tiny patch eccentrically placed. For the sake of those who care for polar expeditions I subjoin the result of this one, a map of the region about the Martian south pole.

Although the action of the polar snows of Mars is largely like our own, there is one peculiarity about their accumulation which finds no adequate parallel on earth. In 1879 and still more markedly in 1881-2. Schiaparelli observed certain strange filaments of snow spread like long ribbons from the north pole in a spiral south-easterly direction. At this time the north polar cap was quite small but gradually these filaments contracted upon it till one day it appeared large and round and the filaments had

vanished. This was some little time after the vernal equinox. From the day this metamorphosis took place, the polar cap proceeded daily to diminish.

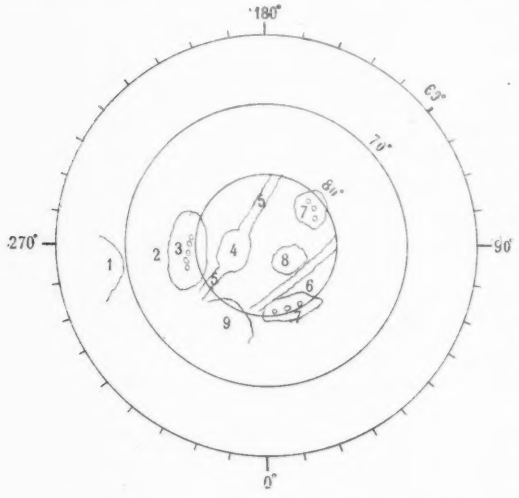
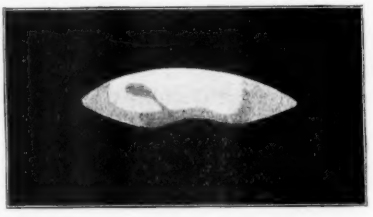


CHART OF THE SOUTH POLE OF MARS.

1. Polar Sea bay; 2, 6, high land; 3, 7, bright spots on high land; 5, 5, rifts; 4, open Polar Sea; 8, position of last snow left; 9, Polar Sea bay at later epoch.

It has been suggested that this phenomenon is what should occur on a planet whose water supply is limited, the percipitation taking place upon an isothermal line that travels with the Sun northward.

There is no evidence here of duplication in any of the canals as yet, a result in keeping with Schiaparelli's observations. With inferior seeing a broad canal may appear double but in better air shows single. An instance of this was observed by Professor Pickering on July 30th in the case of the Ganges which with seeing 4 to 5 (max. 10) be suspected of being double. Later in the evening, the seeing having improved to 9 to 8, it was seen by him single again and very broad. Schiaparelli's observations have a singular knack of turning out true.



Drawing of Mars' South Polar snow cap as seen by W. H. Pickering, July 1, 1894.

Here for the moment we may take leave of the pole and its snows and in our next paper trace as well as we may the course of the water, annually set free, in its journey south.

LOWELL OBSERVATORY, Sept. 12.

PUTTING IN PLACE AND ADJUSTING THE OBJECT GLASSES OF REFRACTING TELESCOPES.

J. A. BRASHEAR.

After the lenses of an objective are thoroughly cleaned by the method given in POPULAR ASTRONOMY for last month the next thing to do is to put on the three pieces of tin foil usually placed on the inner concave curve for the purpose of separating the lenses slightly so as to prevent them from touching one another.

Mark three places equi-distant on the edge of the lens on which the tin-foil is to be placed. For a 4-inch lens cut a strip of tin-foil $\frac{5}{8}$-inch wide and long enough to handle well. Place a drop of mucilage—good quality—on a piece of letter paper and with the finger take up a very small quantity and spread it on the end of the strip of tin-foil, taking care not to cover much more than is to be pasted on the edge of the glass; quickly lay the foil in place letting it lap over about $\frac{1}{16}$-inch for a 4-inch, proportionately more for larger glasses. Lay a piece of clean letter paper over the foil and press down by rubbing with the thumb nail. Let it remain a little while to dry, then slice it off with a sharp knife as our mothers used to slice off the extra dough from the edge of a pie pan. Be sure to trim off the part of the tin-foil that you have pasted over as that paste or mucilage will form an extra thickness which may do an injury. Then put on the second and third pieces in the same way. If neatly done no cleaning is necessary around the foil.

Some of our German friends use strips of a postage stamp, and I believe they would come in first rate if we are willing to break through the conventional tin-foil. I have measured quite a good number of stamps with the gum on them and find the thickness does not vary greatly from twenty-five ten-thousandths of an inch, about one fortieth of a millimetre. This is rather thicker than the tin-foil the writer is in the habit of using for objectives whose inner curves are equal, but with such a slight difference as there would be between the stamps and the tin-foil with its adhesive gum I think it perfectly safe to use the strips of a postage stamp in place of the foil. With all the writer's experience he is never sure that the amount of gum is always the same on the three strips of tin-foil, whereas a careful measurement of a number of stamps did not show a variation of three ten-thousandths of an inch. Columbian or other forms of the longer stamps could be used for larger glasses.

Some objectives I have met with, particularly those of the Frauenhoffer Herschel curves have enough difference in the inner curves to touch in the center. In the absence of a spherometer to measure the curves, this may always be known by looking through one of the lenses at a rather low angle, when Newton's rings will at once be seen at the point of contact of the two lenses. In this case the lenses must be separated by a greater thickness of tin-foil or doubling up of the stamp strips.

The lenses should be placed together carefully. Small lenses may be readily placed together while lying horizontally. Large lenses should always be set on edge and placed together in a vertical position, then firmly grasped with the hands and laid on a cushion, same as used in taking objective out of its cell. The block or book on which the cushion of cheese-cloth is laid should be somewhat smaller than the glass, then the cell will readily slip over it. In some forms of cell, each of the lenses are mounted separately, indeed all our later forms of cells for larger objectives are made this way, so that it is much easier to place the lenses together, but what is written here holds good for any form of mounting.

If the objective is marked to go together a certain way, which is usually the case if it has been corrected locally for irregular density, etc., the lenses should always be replaced in the same manner, at any rate it is safe to place the lenses together as the maker placed them.

In nearly all objectives made up to ten years ago the crown lens was placed in front. In objectives made by Steinheil and the writer the flint lens is placed in front, and it is of course essential that the proper lens should always be placed in the position designed by the maker. If there is a suspicion that the objective is not doing good work because of being wrongly placed (and this has happened more than once), it is an easy matter to reverse it and test it. The story it tells will be quickly understood by the observer if he have only "half an eye."

And now a word about adjusting an objective. Wallaston's method of holding a candle at the center of the eye tube near the focus has been given in a number of excellent works, but I consider it not at all suited to the purpose, and the method of using a sort of spherometer with a telescope attached to set on the objective at the equidistant points and observe the image at center of eyepiece is well enough perhaps for experimental purposes.

The simplest and best way is to put the cell in place on the tube, and observe the image of a bright star, either an artificial

one, say that produced by the Sun shining on the glass insulator of a telegraph line at a sufficient distance to enable it to be brought to a focus, or a first or second magnitude star in the sky. The image of the star should be circular and with a sufficiently high power, say 40 to the inch of aperture, the diffraction rings around it should be true circles and these circles should be concentric with, *i. e.*, equidistant from the central image. If this is not the case the treatment is simple where there are the ordinary three adjusting screws. If the objective has the flint glass front, as in the Steinheil and our own form of objective, the correcting screws should be so operated that that part of the objective toward which the image seems to flare should be *pushed away* from the eye end. If the objective has the crown lens front, the reverse treatment is necessary, *i. e.*, that part of the objective toward which the flare extends should be *drawn toward* the eye end, and the observer should spare no pains until the image is clear and symmetrical. To give the reader an idea of how a perfect star image should look, the following simple experiment will be useful. Lay a spectacle or any convex lens on a place where the Sun will shine on it. Punch a hole through a visiting or similar card, look through the hole in the card at the image produced by the Sun shining on the convex surface,—standing two or three feet away from it. If the hole is punctured with the *point* of the pin the image of both star and its diffraction rings will be quite similar to their ideal appearance in a 2½-inch or 3-inch telescope. A large hole reduces the diffraction rings as in larger glasses and gives one a most excellent idea of how a star ought to appear in a well corrected telescope. I think it is not necessary to consider the colors shown on opposite sides of the stellar image when the adjustments are not perfect, as the diffraction rings are an all sufficient guide, nor is it necessary now to speak of irregular images which are not a function of bad adjustment though they may be of compression of the objective in its cell.

In many of the smaller telescopes the cells are screwed into place. These the maker should always adjust before sending them from the workshop. I have seen it advised to "gently tap the ring holding the cell on the proper side, to adjust the objective," but this is bad advice. If you cannot send it to the maker, send it to a first class mechanic, get the tube mounted on a carefully centered mandril in the lathe and have the objective end refaced until your image is perfect, for without it you cannot expect to do the best work with your glass.

THE FIXED STARS.

W. H. S. MONCK.

IV.

Assuming that the solar system and the fixed stars are both motionless, we should find them all in exactly the same position with regard to each other and to the Sun at the end of a year—that is of a sidereal year. The position of a star is usually determined by its right ascension and declination; and here various possibilities of error creep in. In the first place if in measuring the right ascension we make a mistake as to the exact moment of the transit the star will appear to have moved. The same result would arise from a small error in the precession of the equinoxes, and as regards declination from a slight error in computing the motion of the pole. Measures of declination however are considered somewhat more reliable than those of right ascension; but as regards both elements if the motion indicated is very small but little dependence can be placed on it. Even the direction may be erroneous. Careful observations extending over long periods will no doubt bring small proper motions to light. But early observations do not possess the accuracy of recent ones and can seldom be relied on as to quantity. And the quantities with which we are dealing are in all cases minute. No known star describes an arc of ten seconds per annum on the celestial sphere and only about 300 are known to have motions exceeding half a second. However as they continue (with a few exceptions) to describe equal arcs in the same direction from year to year the result becomes multiplied as time goes on. A star which moves a second in a year will have moved a minute in sixty years, and after careful observations embracing a period of sixty years the fact of its motion and even its quantity would be free from doubt. But if it took sixty years to describe a second instead of a minute we might still be at a loss with regard to its motion.

The sidereal year is the time at the end of which the Sun will occupy the same position with respect to any given fixed star; but in reality this time will be different for each fixed star, for the simple reason that all are moving. We must, therefore, take a number of fixed stars and strike an average; but it may be doubted whether the length of the sidereal year can be computed in this manner with such accuracy as to exclude a small error;

and a very small error would suffice to derange very small proper motions. An under-estimate or over-estimate of the length of the year would apparently confer upon stars small proper motions in increasing or diminishing right ascension. But the motions in question are also liable to be affected by a small error in computing what is known as the precession of the equinoxes, since right assension is measured from the first point of Aries whose position depends on the amount of this precession. These sources of error render small proper motions unreliable.

We cannot distinguish relative from absolute motion by mere inspection. The Copernican system has familiarized us all with this fact, and the aberration of light is really an apparent displacement of the stars due to the motion of the Earth. But may there not be another displacement due not to the motion of the Earth but to that of the solar system? There may be, and I think we may affirm that there is. But all the proper motions—even if we limit ourselves to considerable proper motions which may be relied on—of the stars cannot be referred to this cause. For to account for the observed motions of different fixed stars the Sun would require to be moving in different directions and probably with different velocities also. The motions of the stars in the line of sight as measured by the spectroscope lead to similar results as will be seen hereafter. But notwithstanding this the Sun is not stationary, and a part of the proper motions of the fixed stars is undoubtedly due to its motion. So marked indeed is its influence on the proper motions of the stars that Sir Wm. Herschel deduced the direction of its motion from fourteen stars only and the result did not differ very widely from that arrived at by more elaborate processes. It would probably be impossible to select fourteen stars whose proper motion was large enough to be distinctly ascertainable which would not afford evidence of the Sun's motion provided, that the choice was impartial. If we laid down that on the average one-half of what is called the proper motion of the stars was due to the Sun's motion while the other half represented the real motion of the star, we would not perhaps be very far from the truth; but the proportion is no doubt different in the case of almost every star. What we have to recollect is that the proper motion of a star is a compound made up of the motion of the solar system and the motion of the star.

That stars with large proper motion are comparatively near us seems in any case pretty certain. For supposing the stars to be at rest the effect of the Sun's motion would be *ceteus panbus*

greatest in the case of the nearest stars and would diminish as the stars became more distant. And as regards their real motions unless we ascribe enormous velocities to stars with large proper motion we must assume that they are comparatively near. But the converse does not hold. Stars with small proper motion may also be near us, their real motions being nearly balanced by the effects of the Sun's motion or else nearly reduced to an approaching or receding motion which does not affect the stars' apparent positions on the celestial sphere.

When we know the proper motions of a sufficient number of stars with tolerable accuracy we can deduce the direction in which the Sun is moving, subject to certain qualifications to be stated hereafter. The general effect of the Sun's motion is to make all stars appear to be moving away from that point and towards a point diametrically opposite to it on the sphere. The effect of this motion on the right ascension and declination of stars has to be considered. Let us suppose the right ascension of the point towards which we are moving to be 270°—probably not very far from the truth—then as the stars will appear to move away from this point, all stars whose R. A. lies between 270° and 90° (measuring backwards through 180°) will have a tendency to decrease their right ascensions while all stars whose R. A. lies between 270° and 90° (measuring forwards through 0° or 360°) will have a tendency to increase it. The real motion of the star will of course in many instances overpower this tendency but on taking a large number of stars the effect will be manifest. If, on the average, half of a star's motion in right ascension is due to that of the Sun, three-fourths of the stars in the former interval will have decreasing right ascensions and three-fourths of those in the latter interval increasing right ascensions. This of course supposes that the stars examined are numerous enough to neutralize each other's real motions. Without entering here into the more elaborate methods of finding the direction of the Sun's motion, a close approximation may be thus obtained. Take any large Star Catalogue in order of right ascension and divide it into two equal parts such that the one half presents the largest proportion of motions in increasing right ascension and the other half the largest proportion of motions in decreasing right ascension. The point of division is that towards which (so far as right ascension is concerned) the Sun is moving. As regards declination a somewhat similar method may be pursued. A cursory perusal of any large star catalogue shows that the majority of the stars are receding from the north pole. This shows that the

Sun is moving towards a point in the northern hemisphere. Other considerations might lead us to fix on the particular point. Thus supposing that three-fourths of the stars conformed to the Sun's motion as regards right ascension and that three-fourths also conformed to it as regards declination, a declination of 45° would be indicated for the Sun's goal as it is called. If more than three-fourths conformed to it in declination the goal would probably be farther north than 45°, and if less than three-fourths it would be farther south (the proportion in right ascension being three-fourths throughout). I am here merely explaining the general principles by which the direction of the Sun's motion can be determined though I think it may be doubted whether the more accurate mathematical methods have hitherto yielded more satisfactory results. The fault of course does not lie in the methods but in the data. The most elaborate and accurate mathematical computations are of little real value unless they start from reliable data. Perhaps they rather tend to mislead the reader by supposing that we can obtain results correct to several places of decimals when even the whole numbers are really uncertain.

There are indeed difficulties about ascertaining the exact position of the Sun's goal which I think are for the present insuperable. Small proper motions as already mentioned are liable to be completely overlaid with errors of observation or computation and are thus wholly unreliable. But the exclusive use of stars with large proper motion is objectionable on a different ground. First the nearer stars may have to a certain extent a common drift in space. The Sun may in fact form one of a cluster of stars which are on the whole moving in the same direction though gravitation gives each member a special movement of its own. Of course the Sun might be moving in one direction relatively to such a cluster and in a different direction relatively to the stars external to that cluster. The second consideration is of an opposite character. Large proper motion is more likely to occur when the Sun's motion is additive than when it is subtractive—in other words when the star's real motion is opposite to that of the Sun. Hence among stars with large proper motion there will probably be a tendency to move away from the Sun's goal whereas on general principles we suppose the stars which we are examining to be moving indifferently in all directions. Further still: stars with large proper motion no doubt owe this motion in part to their large actual velocity (or at least to their large actual velocity perdendicular to the line of sight). But the greater the

actual velocity of the star, is the smaller, comparatively speaking, is the effect produced on it by the Sun's motion. Supposing that the star was flying through space at the rate of 200 miles per second while the Sun's rate of motion was only 10 miles per second, it would make very little difference whether the latter tended to increase or to diminish the former. These various causes of error indeed partially neutralize each other. Still they lessen our confidence in the determination of the Sun's goal from the motions of the stars in question.

With regard to small proper motions they appear to be liable to greater errors in right ascension than in declination. The consequence of this is that the effect of the Sun's motion in right ascension is much more completely masked by errors of observation or computation than its effect in declination: and as the effect in declination appears to be greater than that in right ascension the Sun's goal is placed too near the North pole. This effect is of course increased if we select a lower minimum for motions in declination than for those in right ascension as is done for instance in the *Cincinnati Catalogue*. The stars in that Catalogue have on the average greater motions in declination than in right ascension and therefore the effect of the Sun's motion is probably additive in the case of the former motion more frequently than in the latter. This might easily lead to placing the Sun's goal too far north. If observations in right ascension and declination were made with equal accuracy (or inaccuracy) the consequence of errors would be not to displace the position of the goal but to under-rate the velocity with which we are approaching it by partially masking the effect of the Sun's motion in both directions; but if the effects are more decidedly masked in one direction than in the other, the goal will be misplaced. As a matter of fact computations of the position of the Sun's goal based on stars with large and small proper motion seem to give pretty nearly the same right ascension while as regards declination the stars with small proper motion give a more northerly goal. As this is exactly what might have been expected, and the errors in deducing the goal from stars with large proper motion are to a large extent of a compensatory character determinations based on these stars are probably the most reliable. The right ascension of the Sun's goal is not perhaps very far from 270° and the declination not far from 30° or 35° N.

In this mode of computing the direction of the Sun's goal—indeed in all modes of computing it—it is assumed that the stars as a whole are neither moving towards nor away from it. It is in-

deed assumed to a large extent that this is true not merely of the stars as a whole but of the stars in any particular part of the sky. These assumptions may not be true. Thus the galaxy bears at first sight at all events the appearance of a ring. Does this ring revolve, as the late Mr. Jacob Ennis maintained, and if so in which direction? Unfortunately the motion of the galactic stars—at least if we leave out certain stars which are probably *between* us and the galaxy—is small and may be to a large extent disguised by errors of observation. But the question whether all known stars constitute a single system, or whether there are at least two classes—galactic and extra-galactic—is one of the unsolved problems in stellar astronomy. Astronomers have of late favored the latter theory, but I cannot regard the grounds assigned for it as conclusive. Assuming it to be correct, the Sun would probably be found to be moving in a different direction when compared with galactic and with extra-galactic stars. The stars with large proper motion hitherto chiefly used in determining the goal are, on this theory, for the most part extra-galactic. The galactic goal, or goal relative to the galaxy, is therefore at present conjectural. It may be noticed that at the point where the galaxy crosses the equator at about R. A. 285°, the stars ought on the whole to have a southerly motion, owing to the motion of the solar system; but they appear, on the contrary, to have a general drift towards the north. This might arise from a revolution of the galactic stars in the direction from Ophiuchus to Cygnus and thence on to Taurus. But I have not sufficient data to form any positive conclusion on this subject.

The proper motions of the stars bear no fixed relation to their magnitudes. Faint telescopic stars are sometimes moving faster (measured on the celestial sphere) than the brightest in the sky. There is reason indeed to think that the *average* proper motion diminishes with the magnitude of the stars, but apparently not to the extent that might have been expected. Thus assuming that all stars were of the same mass and brilliancy the distances would increase in the ratio of 1.585 for each magnitude, and the average proper motions (measured on the arc) would diminish in the same ratio. It would seem that they diminish in a much smaller ratio than this. This result is no doubt in part due to errors of observation which are more likely to increase than to diminish the amount of small motions. Thus if we took 1000 stars whose motions were really too small to be measured we should probably get proper motions of several times the real amount—some in one direction and some in another—from the impossibility of making

really accurate measurements. That the velocities of the stars really increase with their distance from us is I think very improbable. But the distances may not increase as rapidly as we suppose when we are judging from the intensity of the light only. If there is any intervening medium which intercepts light the distances may increase in a much less ratio than 1.585 for each magnitude, and if further observation should show that the proper motions only diminish in the ratio of 1.3 to 1 (suppose) I think the most rational inference would be that the absorption of light in space is such as to make a distance of 1.3 equal in effect to a distance of 1.585 of pure transparent ether. But when we recollect the great difference in the light-giving power of different stars we shall see the fallacy of attempting to strike averages for the stars of any particular magnitude unless the spectra are identical or the different kinds of spectra are equally distributed among the different classes of stars which we are comparing. It is probable that stars of the Sirian type (a term which will be explained hereafter) are on the average seven times as bright as stars of the Solar type whose masses and distances are equal. Consequently if we are comparing Sirian stars of the first magnitude with Solar stars of the second, we might expect the proper motion of the latter to be on the average greater instead of less than that of the former. It is only with stars that give the same kind of light that we ought to expect a steady decrease in the proper motion as the light becomes fainter. If we are comparing electric lamps with gas lamps when travelling in a train or a ship we cannot expect that their apparent motions will vary with their apparent magnitudes. The electric lamp might look brighter when its distance was double that of the gas lamp and its apparent motion only one-half of that of its compeer. Our knowledge of the electric arc might enable us to say, "this lamp though brighter is farther away," even before measuring its motion. Something of the same kind occurs in the case of the stars. Brightness is a sign of nearness but it is a kind of sign which has to be qualified by the nature of the light. Even without the aid of the spectroscope brilliant white light, especially if there is a greenish or bluish tinge, may be regarded as a caution that the star is not so near as we might expect. Reddish light might be supposed to afford an indication of the contrary but observation renders this doubtful. Perhaps an orange or yellow tint affords the best promise of nearness. But spectroscopic indications are more to be relied on than those of the naked eye.

Besides large proper motion common proper motion in neigh-

boring stars is an indication of comparative nearness. By common proper motion I mean motion in the same direction and with the same velocity. But when motions are very small errors of observation may affect our results so much that we can feel little confidence that the observed common proper motion is real. Hence common proper motion can only be relied on as evidence of nearness to us when the amount of the motion is moderately large. The principle in this case is the following: stars with common proper motion are almost certainly physically connected; and physical connection being a result of gravitation, they cannot be very remote from each other unless we ascribe to them enormous masses. The Sun's distance from the most remote known planet is about 30 times that of the Earth. It is difficult to believe that two stars which exhibit traces of physical connection can be separated by more than 100 times this distance; and therefore if the pair of physically connected stars are pretty widely separated on the celestial sphere we are driven to conclude that their distance from us is (comparatively speaking) moderate. A similar intimation, as has been noticed, is given by double stars easily separable in the telescope yet completing a revolution within a moderate period. Such stars often present the phenomenon of common proper motion when the effects of the revolution have been allowed for. Two indications of comparative nearness are thus combined. I need hardly state that of two pairs of stars separated by equal distances on the celestial sphere those which are (really) nearest to each other are also nearest to us. Remove them to double their actual distance from us and the distance between them on the celestial sphere would be reduced by one-half. In this way we can often reason from nearness to each other to nearness to us and *vice versa*.

THE MOON.

WM. W. PAYNE.

III.

In our last article we gave a brief outline of the history of the study of the Moon to the time of the invention of the telescope. It seems desirable now to call attention to well established facts that have been quite precisely determined since the telescope came to the aid of astronomy.

The Moon must surpass all other celestial objects in interest

for common observers, because it is our nearest neighbor. Ancient observers knew this fact before they were able to determine its distance even approximately. We now know that the mean distance of the Moon from the Earth is 238,840 miles, or 30.135 times the Earth's equatorial diameter; but that its actual distance at any one time may be as much as 15,000 miles greater or less than this value, owing to various changes in the form of the lunar orbit. The shape of the Moon's path with reference to the Earth is that of an ellipse; its nearest point is called perigee, and its most distant point from the Earth is apogee. One-half the sum of these two distances is called the Moon's *mean* distance from the Earth.

The distance from apogee to perigee is also called its major axis, and its length in miles is double the mean distance, or 477,680 miles; a line drawn through the middle point of the major axis and perpendicular to it is called the minor axis of the orbit. It is less in length than the major axis by 3,800 miles approximately. These figures will aid the reader in understanding what is meant by the ellipticity of the Moon's orbit. The term ellipticity or compression should not be confounded with the eccentricity of the orbit.

Let c = ellipticity,
a = semi-major axis,
b = semi-minor axis,
e = eccentricity; then by definition and simple principles of analytic geometry,

$$c = \frac{a - b}{a} \text{ and } e = \sqrt{\frac{a^2 - b^2}{a^2}}$$

If the reader does not understand how the relations of c and e are found, he can readily see that there is a difference between their values, and if he wishes to find the relation between c and e it can be done easily by giving numerical values to a and b.

A revolution of the Moon around the Earth is completed in a little more than 29½ days, during which it passes through all the phases, from new to full, so well known to common observation. It is strange that popular belief still holds that these changes of the Moon, so-called, modify physical conditions of every-day life. Science furnishes no proofs for such opinions.

The Moon's orbit is inclined to the Earth's annual path 5° 8′ and the points of intersection of the two great circles are called nodes. The nodes are not fixed points in the sky, with reference to the stars, but they are in motion, and make a revolution with

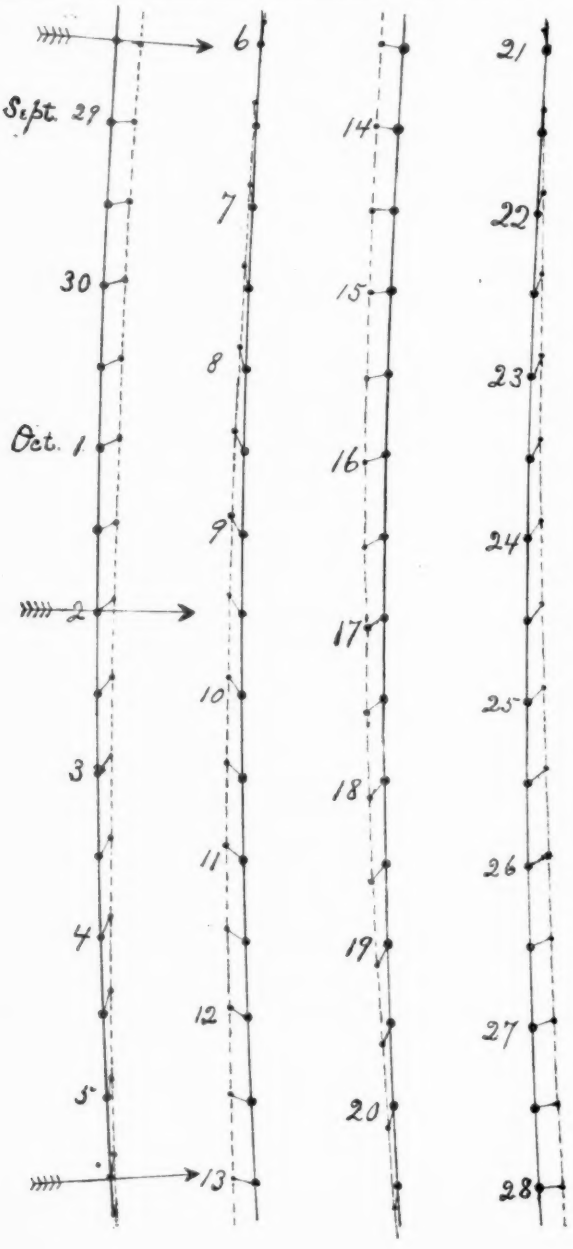
reference to the Earth once in 18½ years, nearly. The various orbital motions of the Moon, their periods and their causes, may be learned from any book on elementary astronomy.

Another thing that may well deserve some attention in this connection is the shape of the Moon's path as related to the Earth's annual orbit around the Sun. As the Moon is constantly near the Earth as compared to the distance of the Sun it must be plain that her path will resemble that of the Earth's during the interval of a year. Those who have studied elementary astronomy easily remember that the path of the Moon with reference to that of the Earth around the Sun is represented as a wavy line alternately crossing it twice each lunar month. But the representations of the relations of the Moon and the Earth in most textbooks are so untrue as to convey a wrong notion of the real shape of the Moon's path in space around the Sun. To give the true idea we have prepared the accompanying cut* which represents respectively the paths of the two bodies for the month of October, 1894. The dotted line is that of the Moon, the other the Earth's path. The arrows point to the Sun. The relative positions of Earth and Moon are indicated for every twelve hours during the month. It will at once be observed that the Moon's path is nowhere convex toward the Sun. It is apparently less concave in that part within the Earth's orbit, than is true pertaining to that portion outside, yet the inside lines are not even straight, much less convex, as is so often represented in figures of common illustration.

Whenever the Moon is at, or quite near, either of the nodes, and they are in line with the Sun and the Earth, eclipses will take place. If the Moon is between the Sun and the Earth the eclipse is solar, and total, or partial, according to the amount of solar surface covered by the Moon. If the Moon is at, or near, the node when on the opposite side of the Earth from the Sun, the eclipse is a lunar one, total or partial, according to the position of the Moon's path through the Earth's shadow. The number of eclipses that may take place in any given period of time depends on the relation of the line of nodes to the relative positions of the Sun and Earth at the beginning of the period. The least number possible during one year is two, both central eclipses of the Sun. The greatest number possible in one year is seven, five of the Sun and two of the Moon.

To an observer on the Earth, the Moon, when full, is perfectly

* The cut we have used is like one which appeared some time ago in the *English Mechanic*.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round, and looks flat, like a disk; but in a small telescope it has the appearance of a great ball, because its spherical form is at once recognized. This can be said only of the one hemisphere of the Moon which faces the Earth, for astronomers have never seen the other, the greater part of which is constantly hidden from view. On account of this fact, the rotation of the Moon on its axis, which is uniform, is nearly identical with its time of revolution around the Earth.

But, because its motion is not uniform in all parts of its orbit, and because its path is inclined to that of the Earth, observers will sometimes see a little more surface on the east side or west side, or in the region of the poles, than at others. This swinging motion of the Moon, with reference to our line of view, is called its libration in latitude and longitude, and enables us to see about four-sevenths of the entire surface of the Moon, while the remaining three-sevenths must be forever invisible. The cause of the libration in longitude is due chiefly to the attractions of the Earth and the Sun.

The diameter of the Moon is 2,163 miles, the error of which is less than one mile. The surface, if spherical, must be about one-thirteenth as great as that of the Earth, and its volume one-forty-ninth. Its mass, however, is only one-eightieth of that of the Earth, because its density is comparatively small, being 3.39 that of water. At first thought it would appear that the Earth and Moon could not have been parts of one great heavenly body at some time in the remote past, as astronomers believe, because they differ so greatly in density now. But this is only a seeming difficulty, for the absolute weight of the materials at the surface of the Earth, and those of the Moon, are nearly the same, as they should be, if from the same origin. The great density of the interior portions of the Earth is the cause of the difference.

Another important feature closely connected with those just mentioned is the force of gravity at the surface of the Moon as compared with that at the surface of the Earth. This is known by the rapidity of the fall of bodies or their weight. Any body on the surface of the Earth will weigh six times as much as it would weigh if transferred to the surface of the Moon. This is due to the difference of attraction at the two surfaces and is equally manifest in the action of many other physical forces. This important fact may well be borne in mind while studying the surface formations of the Moon, occasioned, probably, by volcanic force of surprising magnitude in some very remote time in past lunar history.

Another important physical condition of the Moon depends on the existence of a lunar atmosphere, similar to that which surrounds the Earth. Scholars know that our terrestrial ocean of air surrounds the Earth and has a depth everywhere from one hundred to three hundred miles. They believe that the Moon is destitute of such an atmospheric envelope, and the most conclusive proof that such is the fact is derived from careful and extended observations of the occultation of stars by the Moon. If the Moon possessed an atmosphere like the Earth's, though two thousand times more rare, it could be detected by the appearance of a star as the Moon passed over it, for its time of obscuration would be appreciably too short for the known motion and diameter of the Moon. But an atmosphere at the lunar surface of such rarity would be wholly unappreciable, for the best air pumps of the present time can not reduce the density of the air much more than one thousand times. If there is practically no atmosphere on the Moon, there can be no water, no clouds, no winds, nor storms, as we know them—a changeless solitude! It is easy to see how such a condition as this would make it wholly impossible for beings like ourselves to live on the Moon.

THE REGION OF LACUS SOLIS ON MARS.

J. M. SCHAEBERLE.

During some unusually fine seeing on the morning of Sunday Sept. 2, 1894, at 2:30 A. M., Locus Solis was very clearly shown to be composed of three separate areas. Each area was very dark. The two preceding areas are elongated in a north and south direction and are enveloped and connected by a penumbral shade; the third area (following) is round and quite disconnected from the general mass. The five very small and black circular areas nearer to the planet's equator (shown in the sketch inclosed within the bounding square) were connected by an intensely black and very narrow continuous line which passed centrally through each area, but did not extend to the familiar black circular area from which several so-called "canals" radiate —only the usual faint and diffused marking forming the continuation of the line.

In this connection attention should be called to the fact that in June, 1890, Schiaparelli saw Locus Solis divided into two parts

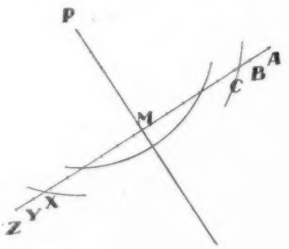
the advancing area being the smaller of the two. (See *La Planéte Mars*, by Camille Flammarion, page 475.)

LICK OBSERVATORY, Sept. 3, 1894.

AN EASY WAY TO MAKE A NOON MARK.

REV. EDWARD RIGGS.

Select a smooth, horizontal surface, as the well planed floor of a veranda, opening toward the south. Borrow a carpenter's spirit-level and make sure that your surface is substantially horizontal. The floor of a room will answer, if a window opens pretty nearly toward the south. Suspend a plumb line at a point, (P), where its shadow will fall on your horizontal surface through a considerable part of the day. Secure this in its place so that it shall not sway, taking care that you do not vitiate its perpendicular position. Fasten a small object, as a small button or a knot of string, on your perpendicular string, at any convenient point where you can watch its shadow. Some sunshiny day, when you are at liberty to attend to it for a little while both morning and afternoon, mark with a pencil point the centre of the shadow of your button. Do this at half a dozen different times before noon, (A, B, C, etc.), at intervals of say half an hour, or a quarter of an hour, making the intervals about equal; and do the same in the afternoon. (. . X, Y, Z) The series of dots may be continued through all the hours that the shadow is falling on the surface, but the essential thing is to have a set of three or four points in succession at some time during the forenoon, say about nine or ten o'clock, and another set at about the same length of time *after* noon, *i. e.*, at about two or three P. M. Join these dots by a carefully drawn line. You will find it very nearly a straight line. Now from the foot of your perpendicular as a centre, draw arcs, with any convenient radius, cutting your dotted line in two places. Bisect the chord that joins these two points of section, and join the middle point (M) of the chord to the foot of your perpendicular. This line, (NP) will be a true north and south line, and when the shadow



* Missionary in Turkey.

of the perpendicular string falls on it, it will be solar noon. From this, by reference to the noon mark column in an almanac, you can get exact mean time.

If it is not convenient to keep a string permanently in the position of your plumb line, select a perpendicular edge that will cast a sharp shadow at noon, as the edge of a square pillar of the veranda, or one side of a window frame, and after satisfying yourself that it is fairly perpendicular, draw exactly from the foot of it a line just parallel to your north and south line, and then the shadow of your solid edge will fall at solar noon.

By comparing the magnetic compass with your north and south line you will be able to observe the amount of the variation of the former, for your particular locality.

GROUPS OF TIME-STARS FOR AMATEUR ASTRONOMERS.

EDNA ILIFF AND LOTTIE WATERBURY.

While making time-observations at the Chamberlin Observatory, it was suggested to us by Dr. Howe that groups might be selected, so as almost to eliminate the errors of azimuth and collimation. The advantage of using such a group is that after the observed time of transit of each star has been corrected for level and diurnal aberration, it may be subtracted from the star's right ascension, and the mean of the remainders will be the clock-correction, with sufficient accuracy, if the instrument is fairly adjusted. Thus time is saved, because it is unnecessary to solve the system of equations, and get the valnes of *a* and *c*.

We therefore first made a list of time-stars, including all those given in the *Berliner Jahrbuch*, and all others in the *American Ephemeris*, except those of extreme southern declination. The *American Ephemeris* gives the apparent positions of a number of stars, of which only the mean places are given in the *Jahrbuch*. For each star in the list we obtained the right ascension and declination for 1895.0, the meridional zenith distance for the latitude of the Chamberlin Observatory (39° 41′) and the azimuth, level, and collimation factors A, B and C.

From this working list groups of four were selected, each group consisting of two pairs, each pair being composed of a star north of the zenith, and one south. The instrument is to be reversed between the pairs of a group. The stars in each group do not succeed each other by an interval of less than three minutes, nor,

if possible, of more than ten minutes. In choosing these groups we were guided by the column of azimuth factors, the object being to get the sum of these, for the stars of a group, very small. Stars having a higher declination than $+60°$ were rejected, except in one instance. We also made groups of two stars, one north and one south, where they were near the zenith; in such a group one star should be observed clamp E., and the other, clamp W.

A list of these groups is given below, in the hope that they may prove useful to amateur astronomers. The right ascension of each star is given for 1895.0, and the outstanding errors of azimuth and collimation, for the mean of each group. The cöefficients of a and c are calculated for latitade 39° 41′, but they vary so slowly that any group which is good for this latitude, will be found satisfactory for any latitude between 38° and 42°.

UNIVERSITY PARK, Colo.

No.	Name.	R. A. h	m	s	Outstanding error for groups.
1	22 Androm..	0	4	52	
	γ Pegasi.....	0	7	50	$-0.01a+0.11c$
	σ Androm...	0	12	51	
	ζ Cassiop....	0	31	7	
2	o Cassiop....	0	38	52	
	δ Androm....	0	41	46	$-0.02a+0.11c$
	γ Cassiop...	0	50	22	
	ε Piscium....	0	57	30	
3	φ Persei.....	1	37	5	
	α Triang.....	1	47	6	$0.00a+0.04c$
	γ Androm...	1	57	27	
	β Triang.....	2	3	18	
4	γ Androm...	1	57	27	
	α Arietis.....	2	1	15	$0.02a+0.09c$
	6 Persei	2	6	37	
	γ Triang....	2	11	4	
5	ξ Persei.....	2	37	2	
	41 Arietis....	2	43	48	$0.00a+0.02c$
	τ Persei.....	2	46	49	
	ε Arietis.....	2	53	12	
6	μ Ceti.....	2	39	16	
	η Persei.....	2	43	2	$0.00a+0.02c$
	τ Persei.....	2	46	49	
	ε Arietis.....	2	53	12	
7	τ Persei.....	2	46	49	
	ε Arietis.....	2	53	12	$0.00a+0.03c$
	ρ Persei.....	2	58	27	
	β Persei.....	3	1	20	
8	γ Persei.....	2	57	11	
	δ Arietis.....	3	5	37	$0.02a+0.03c$
	ζ Arietis.....	3	8	52	
	α Persei.....	3	16	50	
9	ρ Persei.....	1	58	27	
	β Persei.....	3	1	20	$0.03a+0.00c$
	ζ Arietis.....	3	8	52	
	α Persei.....	3	16	50	
10	24 Camel....	3	20	34	
	f Tauri.....	3	25	4	$-0.03a+0.11c$
	δ Persei.....	3	35	27	
	17 Tauri.....	3	38	38	
11	4 Camel.....	4	39	15	
	i Tauri.....	4	45	14	$-0.01a+0.09c$
	ι Aurigæ.....	4	50	9	
	ζ Aurigæ.....	4	55	8	
12	η Aurigæ.....	4	59	9	$0.00a+0.03c$
	μ Aurigæ.....	5	6	14	
13	ζ Tauri.....	5	31	22	
	o Aurigæ.....	5	37	46	$0.01a+0.04c$
	130 Tauri....	5	41	19	
	δ Aurigæ.....	5	50	53	
14	β Aurigæ....	5	51	50	
	ν Orionis.....	6	1	35	$0.00a+0.15c$
	2 Lyncis.....	6	10	22	
	μ Gemin.....	6	16	36	

No.	Name.	R. A. h	m	s	Outstanding error for groups.
15	ψ Aurigæ.....	6	16	49	
	γ Gemin.....	6	22	44	$-0.00a+0.02c$
	51 Aurigæ...	6	31	23	
	ψ^3 Aurigæ...	6	39	10	
16	ψ^5 Aurigæ...	6	39	10	
	18 Mono.....	6	42	23	$0.00a+0.19c$
	Σ Gemin.....	6	45	52	
	15 Lyncis....	6	48	11	
17	ψ^5 Aurigæ...	6	39	10	$0.01a+0.09c$
	Σ Gemin.....	6	45	52	
18	63 Aurigæ...	7	4	26	
	64 Aurigæ...	7	10	44	$0.00a+0.04c$
	19 Lyncis....	7	14	18	
	ζ Canis Maj.	7	21	27	
19	63 Aurigæ...	7	4	26	$-0.02a+0.02c$
	64 Aurigæ...	7	10	44	
20	β Gemino....	7	38	53	
	26 Lyncis....	7	47	4	$-0.02a+0.03c$
	ω^1 Canci....	7	54	35	
	27 Lyncis....	8	0	33	
21	27 Lyncis....	8	0	33	
	ξ Canci.....	8	6	11	$0.00a+0.00c$
	31 Lyncis....	8	15	39	
	Gr. 1450.....	8	26	5	
22	31 Lyncis....	8	15	39	$-0.03a+0.05c$
	Gr. 1450.....	8	26	5	
23	31 Lyncis....	8	15	39	
	Gr. 1450.....	8	26	5	$-0.02a+0.02c$
	Gr. 1460.....	8	31	31	
	δ Canci.....	8	38	43	
24	η Canci.....	8	26	38	
	Gr. 1460.....	8	31	31	$-0.02a+0.02c$
	υ Canci.....	8	40	21	
	ι Urs. Maj....	8	52	1	
25	Gr. 1460.....	8	31	31	
	δ Canci.....	8	38	43	$-0.02a+0.01c$
	σ^2 Canci.....	8	47	50	
	ε Urs. Maj....	8	52	1	
26	i Urs. Maj...	8	52	1	
	$\varkappa$ Canci.....	9	2	4	$0.00a+0.08c$
	40 Lyncis....	9	14	40	
	Σ Urs. Maj..	9	25	50	
27	α Canci.....	8	52	45	
	Gr. 1501.....	8	56	19	$0.01a+0.04c$
	36 Lyncis....	9	6	56	
	40 Lyncis....	9	14	40	
28	36 Lyncis....	9	6	56	$-0.03a+0.06c$
	38 Lyncis....	9	12	19	

No.	Name.	R. A. h	m	s	Outstanding error for groups.
29	36 Lyncis....	9	6	56	$0.00a+0.08c$
	40 Lyncis....	9	14	40	
30	Σ Urs. Maj..	9	25	50	
	o Leonis.....	9	35	33	$0.00a+0.08c$
	υ Urs. Maj..	9	43	31	
	π Leonis.....	9	54	40	
31	υ Urs. Maj..	9	43	31	
	μ Leonis.....	9	46	48	$0.01a+0.18c$
	19 Leo. Min.	9	51	15	
	α Leonis.....	10	2	47	
32	μ Urs. Maj..	10	16	4	
	31 Leo. Min.	10	21	49	$0.02a+0.07c$
	37 Urs. Maj.	10	28	24	
	33 Sextantis	10	36	4	
33	μ Ursæ Maj.	10	16	4	$0.00a+0.05c$
	31 Leo. Min.	10	21	49	
34	36 Urs. Maj.	10	23	54	
	33 Sextantis	10	36	4	$0.02a+0.01c$
	l Leonis.....	10	43	44	
	β Urs. Maj..	10	55	30	
35	37 Urs. Maj.	10	28	24	
	33 Sextantis	10	36	4	$0.01a+0.01c$
	l Leonis.....	10	43	44	
	β Urs. Maj..	10	55	30	
36	β Urs. Maj..	10	55	30	
	α Leonis.....	10	59	36	$-0.01a+0.06c$
	ψ Urs. Maj..	11	3	46	
	ν Urs. Maj..	11	12	49	
37	ψ Urs. Maj..	11	3	46	$0.00a+0.06c$
	ν Urs. Maj..	11	12	49	
38	β Leonis.....	11	43	42	
	γ Urs. Maj..	11	48	18	$-0.02a+0.03c$
	o Virginis....	11	59	52	
	δ Urs. Maj...	12	10	14	
39	γ Urs. Maj..	11	48	18	
	π Virginis...	11	55	30	$0.01a+0.04c$
	o Virginis...	11	59	52	
	δ Urs. Maj..	12	10	14	
40	2 Can. Ven..	12	10	52	$-0.02a+0.02c$
	6 Can. Ven..	12	20	41	
41	δ Urs. Maj..	12	10	14	
	η Virginis....	12	14	32	$0.01a+0.06c$
	6 Can. Ven..	12	20	41	
	8 Can. Ven..	12	28	45	
42	δ Urs. Maj..	12	10	14	
	η Vriginis ...	12	14	32	$0.02a+0.02c$
	74 Urs. Maj.	12	25	3	
	γ Virginis...	12	36	20	

No.	Name.	R. A. h	m	s	Outstanding error for groups.
43	2 Can. Ven..	12	10	52	
	η Virginis...	12	14	32	$-0.01a+0.02c$
	6 Can. Ven..	12	20	41	
	74 Urs. Maj.	12	25	3	
44	6 Can. Ven..	12	10	41	$-0.03a+0.02c$
	8 Can. Ven..	12	28	45	
45	ε Urs. Maj..	12	49	25	
	ε Virginis....	12	56	57	$-0.02a+0.05c$
	17 Can. Ven.	13	5	14	
	20 Can. Ven.	13	12	50	
46	17 Can. Ven.	13	5	14	
	20 Can. Ven.	13	12	50	$0.04a+0.04c$
	ζ Urs. Maj..	13	19	42	
	ζ Virginis....	13	29	21	
47	17 Can. Ven	13	5	14	$-0.01a+0.01c$
	20 Can. Ven.	13	12	50	
48	*d* Bootis.....	14	5	37	
	λ Bootis.....	14	12	24	$-0.02a+0.06c$
	$\mathcal{S}$ Bootis.....	14	21	37	
	ρ Bootis.....	14	27	18	
49	γ Bootis.....	14	27	51	
	ε Bootis.....	14	40	24	$-0.02a+0.15c$
	Gr. 2164.....	14	48	46	
	P XIV 221...	14	51	16	
50	109 Virginis.	14	40	56	
	Gr. 2164.....	14	48	46	$-0.06a+0.12c$
	β Bootis.....	14	57	59	
	δ Bootis.....	15	11	16	
51	β Bootis.....	14	57	59	
	δ Bootis.....	15	11	16	$0.03a+0.02c$
	μ Bootis.....	15	20	31	
	ν^1 Bootis....	15	27	9	
52	μ Bootis.....	15	20	31	$0.00a+0.02c$
	ν^1 Bootis....	15	27	9	
53	μ Bootis.....	15	20	31	$0.00a+0.02c$
	ν^1 Bootis.....	15	28	1	
54	μ Bootis.....	15	20	31	$0.06a+0.02c$
	φ Bootis.....	15	34	3	
55	γ Serpentis.	15	51	36	
	Gr. 2296.....	15	55	18	$0.00a+0.04c$
	$\mathcal{S}$ Draconis..	15	59	55	
	δ Ophiuchi..	16	8	52	
56	σ Cor. Bor..	16	10	45	
	τ Herculis...	16	16	35	$-0.02a+0.03c$
	Gr. 2343.....	16	22	8	
	λ Ophiuchi..	16	25	37	

No.	Name.	R. A. h	m	s	Outstanding error for groups.
57	γ Herculis...	16	17	17	
	Gr. 2343.....	16	22	8	$-0.01a+0.07c$
	σ Herculis....	16	30	43	
	ζ Herculis....	16	37	20	
58	σ Herculis....	16	30	43	$-0.03a+0.04c$
	η Herculis....	16	39	18	
59	σ Herculis....	16	30	43	
	η Herculis....	16	39	18	$-0.03a+0.05c$
	Gr. 2377.....	16	43	18	
	φ Ophiuchi..	16	52	42	
60	σ Herculis....	16	30	43	
	η Herculis....	16	39	18	$-0.05a+0.06c$
	Gr. 2377.....	16	43	18	
	49 Herculis..	16	47	18	
61	ε Herculis.. .	16	56	16	
	Gr. 2415.....	17	4	21	$-0.01a+0.07c$
	π Herculis...	17	11	23	
	χ Herculis...	17	23	57	
62	ι Herculis....	17	36	30	
	η Herculis...	17	42	21	$0.01a+0.07c$
	ξ Dracon.....	17	51	43	
	72 Ophiuchi.	18	2	22	
63	α Lyræ.....	18	33	23	$0.01a+0.01c$
	ε Lyræ.....	18	40	52	
64	ε Lyræ.....	18	40	52	
	β Lyræ.....	18	46	12	$0.05a+0.02c$
	R Lyræ.....	18	52	8	
	γ Lyræ.....	18	55	1	
65	β Lyræ.....	18	46	12	$0.02a+0.09c$
	R Lyræ.....	18	52	8	
66	R Lyræ.....	18	52	8	$-0.01a+0.08c$
	ι Lyræ.....	19	3	33	
67	δ Sagittæ....	19	42	42	
	ψ Cygni.....	19	52	55	$0.02a+0.03c$
	τ Aquilæ.....	19	59	1	
	33 Cygni.....	20	10	58	
68	α Cygni.....	20	37	51	
	ε Cygni.....	20	41	58	$0.04a+0.03c$
	32 Vulpec.....	20	50	5	
	ζ Cygni.....	21	1	7	
69	α Cygni.....	20	37	51	$0.00a+0.05c$
	ε Cygni.....	20	41	58	
70	α Cygni.....	20	37	51	
	ε Cygni.....	20	41	58	$0.00a+0.01c$
	ν Cygni.....	20	53	16	
	61 Cygni.....	21	2	11	

No.	Name.	R. A. h	m	s	Outstanding error for group.
71	ν Cygni	20	53	16	$0.00a+0.03c$
	61 Cygni	21	2	11	
72	ζ Cygni	21	1	7	$0.03a+0.15c$
	τ Cygni	21	10	36	
	α Cephei	21	16	4	
	ζ Capricorni	21	20	40	
73	ζ Cygni	21	1	7	$-0.02a+0.09c$
	τ Cygni	21	10	36	
74	61 Cygni	21	2	11	$-0.03a+0.20c$
	Gr. 3415	21	9	8	
	g Cygni	21	25	34	
	ξ Aquarii	21	32	10	
75	τ Cygni	21	10	36	$-0.06a+0.09c$
	9 Cygni	21	25	34	
76	13 H Ceph.	21	35	42	$0.00a+0.05c$
	E Pegasi	21	39	2	
	π^2 Cygni	21	42	55	
	16 Pegasi	21	48	17	
77	31 Pegasi	22	16	21	$0.02a+0.07c$
	3 Lacertæ	22	19	26	
	δ Cephei	22	25	16	
	ζ Pegasi	22	36	14	
78	31 Pegasi	22	16	21	$-0.03a+0.05c$
	3 Lacertæ	22	19	26	
	7 Lacertæ	22	26	58	
	10 Lacertæ	22	34	33	
79	10 Lacertæ	22	34	33	$-0.01a+0.03c$
	13 Lacertæ	22	39	24	
80	μ Pegasi	22	44	56	$0.01a+0.15c$
	o Androm.	22	57	5	
	Br. 3077	23	8	14	
	τ Pegasi	23	15	26	
81	72 Pegasi	23	28	45	$0.01a+0.06c$
	λ Androm.	23	32	25	
	ρ Cassiop.	23	49	8	
	ω Piscium	23	53	55	
82	72 Pegasi	23	28	45	$0.04a+0.11c$
	χ Androm.	23	35	14	

THE POLAR SNOWS AND NON-GLACIATION OF MARS.

J. R. HOLT.

I am only acquainted with three hypotheses which seek to account for the rapid and almost complete melting of the polar snows, so far exceeding anything similar known on the Earth. These are, (1) that the atmospheric pressure being different, the melting point of snow is different also; (2) that the snows, although covering a large area, are thin, so that the actual volume is not great; (3) that they are not snow at all, but something resembling it.

Now as to (1) the melting point of snow is not much affected by pressure; to cause a substantial lowering, say to — 5° C., would require an increase in atmospheric density which is simply preposterous (about 675 atmospheres, see Maxwell's *heat* "lowering of freezing point by pressure"); besides which the melting point of ice is *raised* by a diminution of pressure, and as all the evidence points to the atmosphere of Mars being *less* dense than ours, the melting point of the Martian snows ought to be very (not .01°) slightly *higher* than ours.

As to (2) the suggestion is probable enough, and most likely does play a considerable part in causing the phenomenon; but

the extensive changes which follow the melting, especially at the south pole, seem to involve the setting free no inconsiderable body of water.

As to (3) it may be remarked that the substance appears to resemble snow very closely, and the product of its melting seems identical with the substance composing the Martian oceans.

I have a suggestion to offer; which seems so obvious that I fancy it must have been put forward before.

On the Earth we know that the winter and summer extremes of temperature are mitigated by the ocean, the summer heat being stored up in the water, and the winter temperature thereby raised. On Mars there is no large body of water to act like our ocean; so the annual range of temperature must be very great; far exceeding anything known here. Hence it is conceivable, that even if the mean be considerably below ours, the summers may be hotter. Then taking their greater length into account, a much greater melting effect on the snows is to be expected.

The matter of non-glaciation is one of extreme difficulty, and I cannot offer any plausible explanation; at the same time I ought certainly to allude to it, if only to note that there is an unexplained difficulty.

Sir R. Ball in "The Cause of an Ice Age" gives as conditions for glaciation of a hemisphere, great eccentricity of orbit and summer in perihelion. Now the greatest eccentricity of the Earth's orbit was about 0.071; the present eccentricity of the orbit of Mars is about 0.093, and the S. hemisphere has its summer in perihelion. It ought then to be glaciated, but it evidently is not, and this is the difficulty referred to.

It may be said that the two causes mentioned are not in themselves sufficient, that they must be aided by the distribution of sea and land. But this is not a fair way of putting it; it would be more accurate to say that the ocean exerts a compensating influence, which may be sufficient under suitable circumstances to prevent glaciation, even when the astronomical conditions are most favorable, and there will be no glaciation unless the distribution of sea and land is such as to impede this compensating action.

Thus put, I find it impossible to believe that the most favorable conditions which *could* have prevailed on the Earth, equaled those at present prevailing on Mars.

To elucidate this, I will go somewhat into detail.

Take the N. hemisphere of the Earth. At present (Cause of an Ice Age, p. 102) the mean daily Sun heat during the summer of 186 days is 1.24, during the winter of 179 days 0.75, the average heat received in a day being 1. This unit of heat raises the temperature from the point to which it would sink if the Sun were removed, to the mean annual temperature (59°.9 F. or 15°.5 C. Scott's "Elementary Meteorology" p. 231). What the first point is we cannot exactly say; it is sometimes called the temperature of space, but I do not feel sure that the ether of space *can* possess

a temperature. In any case it is probably not much above the absolute zero, or — 273° C. So that our unit of heat corresponds to an increase of 288° C. Taking it as only 250°, to be on the safe side, we see that the difference between the winter and summer supplies of heat amounts to 122°. As a matter of fact there is no such enormous range anywhere. Even at Werchojansk, where it is greatest, the mean annual range is only about 64° C., while over the greater part of the hemisphere it does not exceed 12° C. The average is certainly not above 25° C. Hence, under present circumstances, the compensation amounts to at least 97°. In the glacial period the difference was .70, corresponding to a range of 175°. The compensation at present existing would reduce this to 78°. But of course if the present distribution of land and sea existed at the glacial epoch, the ocean currents, etc., would gain greater strength, and the compensation would be greater. In fact it would probably be more accurate if stated like a proportion, viz., when the difference is .49 the range is 25° or less, so when the difference was .70 the range can hardly have much exceeded 36°. This would hardly be sufficient to cause glaciation over the entire hemisphere, but it probably would cause a considerable extension of the polar ice-cap, and if the distribution of sea and land was such as to cut off the warm ocean currents, the actual extension of the ice may be easily accounted for.

Now let us examine the case of Mars. For the south hemisphere the corresponding figures are 1.42 in summer for 306 days, and .66 in winter for 381 days. This is much more extreme than anything which can have taken place on the Earth. Unless the compensation be greater it would seem that this hemisphere ought to be glaciated. But can the compensation be greater? On the Earth it is mainly threefold. (1). The ocean absorbs heat in summer, and gives it out in winter; (2) ocean currents transport heat from the tropics, and the warm hemisphere, to the cold; (3) atmospheric currents convey aqueous vapor from the warm hemisphere to the cold, and this vapor gives out most of its heat in freezing. Now on Mars the first two of these must be nearly *nil*, and the third, although very possibly greater than on the Earth, cannot equal all three together.

So instead of the compensation being greater than on the Earth, it seems to be certainly less; moreover it is not only less than that existing at present, but less than that in the glacial period, because, although we may suppose the distribution of sea and land to be such as to reduce the ocean currents to nothing, and so make (2) *nil*, the ocean itself remains, and consequently (1). (1) would not become *nil* unless the hemisphere was almost entirely land in the glacial period, and we know it was not. And yet the south hemisphere of Mars is not glaciated. How shall we explain this anomaly?

I cannot offer any adequate solution. I can only throw out a few suggestions.

The first is that the temperature of Mars may be much higher than is supposed, or than its distance from the Sun would lead us

to think. I attempted to calculate it from the distance from the Sun, and the observed height of the clouds, on the assumption that the humidity of the atmosphere is about equal to ours. The elements of the estimate are nearly all uncertain, but my result was that it cannot be higher than 60° C., and is probably not lower than — 60° C. An average of 30° or 40°, although improbable, is quite consistent with the conditions.

Secondly, possibly a *very* great annual range may not be favorable to glaciation. A temperature not very much below 0° will cause the greater part of the vapor in the air to be thrown down as snow, and a considerable lowering of temperature will cause little more to be deposited. It is true that the snow will be very much colder, but merely raising it again to 0° requires a comparatively small amount of heat in comparison with its liquefaction.

Thirdly, long seasons may not be favorable. Although the winter is longer than the summer, the latter is as long as 10 of our months, and the fierce heat poured in during that prolonged time may be adequate to melting the snow formed during the winter.

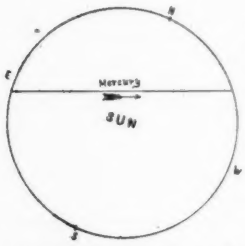
Fourthly, it is not improbable that the onset of glaciation may be somewhat sudden. The accumulated ice itself may block and divert the ocean currents, and, when it covers the sea, may reflect back most of the incident heat, so as to prevent the ocean from storing it up. All the conditions being fairly favorable for glaciation, it yet may not occur, the excess of ice formed each winter being melted each summer, till at last an exceptionally cold summer occurs; the beginning of glaciation is thus started by the lessened amount of ice melted, and once started it goes on rapidly increasing. So glaciation not only requires favorable conditions, but also something to start it; now nothing to start it may have occurred on Mars.

The subject of glaciation is of course, closely connected with the melting of the polar snows; but the two are not quite identical, and so I judged it better to treat them separately. Of course as long as the ice formed during the winter all melts away during the summer, no glaciation can occur; it is only when the winter's ice is not all melted during the summer, that the ice-cap begins to increase from year to year, and the beginning of a glacial period sets in.

In treating of the melting of the snows, I suggested that a considerable part is played by the Polar Sea. Of course if there is no sea surrounding the north pole, this suggestion cannot apply; but we may remark that in that case the intensity of the seasons will be even greater, and the first cause suggested will have more force.

In speaking of the doubtful areas, I attributed considerable importance to the excessive saltness of the sea, causing it to leave a saline incrustation on the rocks. This is not *essential* to my hypothesis. Briefly, the latter may be stated thus: the doubtful areas are possibly archipelagos of small rocks; when these rocks are wet, they are dark and look very like sea; when they are dry, they look like land.

PLANET NOTES FOR NOVEMBER.



Mercury will be at inferior conjunction Nov. 10 at $12^h\ 34^m$ P. M. central standard time. The declinations of Sun and Mercury differ by only 4′ 53″so that the planet will be seen projected on the face of the Sun. The transit will last a little over five hours, beginning at $9^h\ 55^m$ A. M. and ending at $3^h\ 12^m$ P. M. central time. For more accurate predictions see the note on "The Transit of Mercury" on another page. The accompanying cut shows the apparent course which Mercury will take across the solar disc.

We hope that most of our readers will have the opportunity to witness this event. The best way for most to observe it will probably be by projecting the Sun's image on a white screen. Such a screen may be made of white cardboard and fastened a foot or more back of the eyepiece of the telescope by means of a wire frame. By proper focusing a very sharp image of the Sun, from six inches to a foot or more in diameter, may be obtained even with a very small telescope or spy-glass.

On the 11th at $10^h\ 21^m$ A. M. Mercury will pass by Venus, only 8′ south of the latter. On the 27th at $10^h\ 58^m$ A. M., Mercury will be at greatest elongation west from the Sun, 20° 10′. He will be at greatest brilliancy as morning planet, Nov. 26.

Venus will be at superior conjunction Nov. 30, at $9^h\ 17^m$ A. M., being then directly behind the Sun. She will not be in good position for observation during the month.

Mars has for some time been the most conspicuous object, save the Moon, in the evening sky. He far out-ranks the first magnitude stars in brilliancy, appearing almost to have a disc visible to the naked eye. Having in October passed his point of nearest approach to the Earth, he is still comparatively near and in very favorable position for observation by amateurs. He will be in conjunction with the Moon, 3° south of the latter, Nov. 9 at $12^h\ 56^m$ A. M. On the 22d he will reach the end of the westward loop in his apparent path among the stars and will then begin to move eastward.

Jupiter lights up the eastern half of the sky while Mars does the western. The two planets are nearly equal in brilliancy but quite different in color, the silvery hue of Jupiter contrasting strongly with the ruddy light of Mars. Jupiter is in good position for observation after midnight. He will be in conjunction with the Moon Nov. 16 at $4^h\ 04^m$ A. M.

Saturn and *Uranus* will be behind the Sun during November.

Neptune may be observed all night, the best time being about midnight when the planet is near the meridian. He is in Taurus not far from the star *l*.

Planet Tables for November.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.

Date.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Nov. 5.....	15 26.4	− 20 22	7 46 A. M.	12 26.5 P. M.	5 07 P. M.
15.....	14 43.1	− 14 10	5 03 "	11 03.9 "	5 04 "
25.....	14 47.6	− 13 34	5 20 "	10 29.2 "	3 38 "

VENUS.

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Nov. 5.....	14 20.6	− 12 54	6 09 A. M.	11 20.8 A. M.	4 32 P. M.
15.....	15 09.9	− 16 57	6 37 "	11 30.6 "	4 25 "
25.....	16 01.1	− 20 16	7 04 "	11 42.6 "	4 21 "

MARS.

Nov. 5.....	1 26.6	+ 7 52	3 51 P. M.	10 25.0 P. M.	4 59 A. M.
15.....	1 20.4	+ 7 54	3 05 "	9 39.6 "	4 14 "
25.....	1 19.5	+ 8 21	2 23 "	8 59.2 "	3 36 "

JUPITER.

Nov. 5.....	6 26.5	+23 00	7 46 P. M.	3 28.0 A. M.	11 11 A. M.
15.....	6 24.1	+23 02	7 04 "	2 46.4 "	10 29 "
25.....	6 19.9	+23 06	6 16 "	1 59.0 "	9 42 "

SATURN.

Nov. 5.....	13 54.3	− 9 19	5 28 A. M.	10 54.5 A. M.	4 21 P. M.
15.....	13 58.7	− 9 43	4 55 "	10 20.6 "	3 45 "
25.....	14 03.2	− 10 06	4 22 "	9 45.6 "	3 09 "

URANUS.

Nov. 5	14 52.6	− 16 11	7 07 A. M.	12 04.5 P. M.	5 02 P. M.
15	14 54.8	− 16 21	6 19 "	11 16.0 A. M.	4 13 "
25	14 57.4	− 16 32	5 44 "	10 39.6 "	3 36 "

NEPTUNE.

Nov. 5	4 56.4	+21 07	6 21 P. M.	1 54.3 A. M.	9 27 A. M.
15	4 55.4	+21 05	5 41 "	1 13.9 "	8 47 "
25	4 54.3	+21 03	5 01 "	12 33.5 "	8 06 "

THE SUN.

Nov. 5	14 43.6	− 15 51	6 44 A. M.	11 43.7 A. M.	4 44 P. M.
15	15 24.0	− 18 38	6 57 "	11 44.8 "	4 32 "
25	16 05.9	− 20 52	7 10 "	11 47.3 "	4 25 "

Phases and Aspects of the Moon.

		Central Time. d	h m
Apogee	Nov.	4	4 00 P. M.
First Quarter		5	9 16 A. M.
Full Moon		13	1 49 A. M.
Perigee		16	2 36 P. M.
Last Quarter		19	8 08 P. M.
New Moon		27	2 54 A. M.

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Geselischaft".]

MAXIMA.

Nov.	2	T Herculis.
	4	R Lyræ.
	5	R Ophiuchi.
	5	U Geminorum.
	6	S Cassiopeiæ.
	8	U Capricorni.
	9	T Ophiuchi.
	11	S Piscium.
	13	T Capricorni.
	21	R Cancri.
	21	U Arietis.

MAXIMA CONT.

	23	R Capricorni.
	26	S Orionis.

MINIMA.

Nov.	2	R Boötis.
	3*	R Sagittarii.
	4	S Sagittarii.
	8	V Tauri.
	11	S Vulpeculæ.
	12	R Ceti.
	13	W Scorpii.
	14	R Tauri.

MINIMA CONT.

	15	L^2 Puppis.
	18	R Leonis.
	19	U Cygni.
	20	S Tauri.
	20	V Geminorum.
	23	X Libræ.
	26	S Coronæ.
	27	S Ursæ Majoris
	27	U Monocerotis.
	28	S Leonis.
	29†	V Boötis.
	30	S Carini.
	30	R Scuti.

* The "Vierteljahrsschrift" gives this as a maximum.
† The "Vierteljahrsschrift" gives this as a maximum.

Occultations Visible at Washington.

Date 1894		Star's Name.	Magnitude.	IMMERSION Washington M. T. h m	IMMERSION Angle from N p't. °	EMERSION Washington M. T. h m	EMERSION Angle from N p't. °	Duration. h m
Nov.	3	ω Sagittarii	5	9 11	134	9 35	179	0 24
	7	h^3 Aquarii	7	10 44	85	11 43	201	0 59
	7	h^4 Aquarii	8	11 42	87	12 38	204	0 56

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

Alternate Minima.

		h
Nov.	2	2 P. M.
	7	1 "
	12	1 "
	17	1 "
	22	12 M.
	27	12 "

ALGOL.

Alternate Minima.

Nov.	2	1 A. M.
	7	6 P. M.
	13	12 M.
	19	5 A. M.
	24	11 P. M.
	30	5 "

λ TAURI.

Alternate Minima.

Nov.	5	4 P. M.
	13	2 "

λ TAURI CONT.

Alternate Minima.

		h
	21	11 A. M.
	29	9 "

R. CANIS MAJORIS.

Every third minimum.

Nov.	3	10 A. M.
	6	8 P. M.
	10	6 A. M.
	13	3 P. M.
	17	1 A. M.
	20	11 A. M.
	23	9 P. M.
	27	7 A. M.
	30	4 P. M.

S. CANCRI.

Nov.	8	4 A. M.
	17	3 P. M.
	27	3 A. M.

S. ANTLIÆ.

Every tenth minimum.

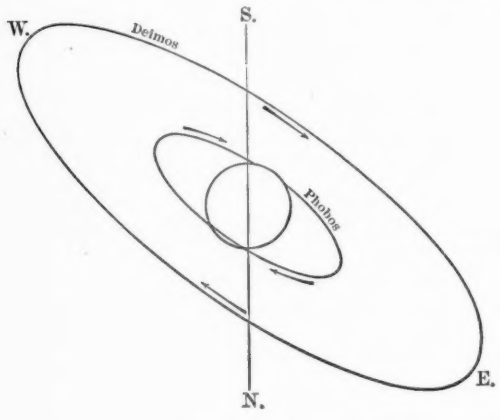
		h
Nov.	2	2 A. M.
	5	8 A. M.
	8	2 P. M.
	11	8 P. M.
	15	2 A. M.
	18	7 "
	21	1 P. M.
	24	7 P. M.
	28	1 A. M.

Y CYGNI.

Every fourth minimum.

Nov.	3	7 A. M.
	9	7 "
	15	7 "
	21	7 "
	27	7 "

The Satellites of Mars.



DEIMOS.

		h	
Nov.	1	4.5 A. M.	W.
	3	1.9 "	E.
	4	11.3 P. M.	W.
	6	8 7 "	E.
	8	6.2 "	W.
	10	3.6 "	E.
	12	1.0 "	W.
	14	10.4 A. M.	E.
	16	7.8 "	W.
	18	5.1 "	E.

PHOBOS.

Nov.	1	3.2 A. M.	W.
	2	6.0 "	E.
	3	8.8 "	W.
	4	11.6 "	E.
	5	2.4 P. M.	W.
	6	5.1 "	E.
	7	7.9 "	W.
	8	10.7 "	E.
	10	1.5 A. M.	W.
	11	4.3 "	E.
	12	7.1 "	W.

For Phobos the central time of every seventh eastern and western elongation is gven, and for Deimos every third; the intermediate ones may te found by adding the periodic time of each satelliite Periodic time of Phobos 7h 39m.2. Periodic time of Deimos 1d 6h 17m.9.

Phenomena of Jupiter's Satellites.

Central Time.

		h	m			
Nov.	1	3	11 P. M.	II	Oc.	Re.
		10	35 "	I	*Sh.	In.
		11	41 "	I	*Tr.	In.
	2	12	51 A. M.	I	*Sh.	Eg.
		1	57 "	I	*Tr.	Eg.
		7	46 P. M.	I	*Ec.	Dis.
		11	04 "	I	*Oc.	Re.
	3	4	32 A. M.	II	*Sh.	In.
		6	42 "	II	Tr.	In.
		7	07 "	II	Sh.	Eg.
		9	18 "	II	Tr.	Eg.
		4	04 P. M.	I	Sh.	In.
		6	08 "	I	Tr.	In.
		7	20 "	I	Sh.	Eg.
		8	24 "	I	*Tr.	Eg.
		11	10 "	III	*Ec.	Dis.
	4	1	42 A.M.	III	*Ec.	Re.
		3	21 "	III	*Oc.	Dis.
		6	08 "	III	Oc.	Re.
		2	14 P. M.	I	Ec.	Dis.
		5	31 "	I	Oc.	Re.
		11	42 "	II	*Ec.	Dis.
	5	4	22 A. M.	II	*Oc.	Re.
		11	32 "	I	Sh.	In.
		12	35 P. M.	I	Tr.	In.
		1	48 "	I	Sh.	Eg.
		2	51 "	I	Tr.	Eg.
	6	8	42 A. M.	I	Ec.	Dis.
		11	58 "	I	Oc.	Re.
		5	50 P. M.	II	Sh.	In.
		7	53 "	II	*Tr.	In.
		8	24 "	II	*Sh.	Eg.
		10	29 "	II	*Tr.	Eg.
	7	6	01 A. M.	I	Sh.	In.
		7	02 "	I	Tr.	In.
		8	16 "	I	Sh.	Eg.
		9	18 "	I	Tr.	Eg.
		1	18 P. M.	III	Sh.	In.
		4	00 "	III	Sh.	Eg.
		5	20 "	III	Tr.	In.
		8	07 "	III	*Tr.	Eg.
	8	3	11 A. M.	I	*Ec.	Dis.
		6	25 "	I	Oc.	Re.
		12	59 P. M.	II	Ec.	Dis.
		5	33 "	II	Oc.	Re.
	9	12	29 A. M.	I	*Sh.	In.
		1	28 "	I	*Tr.	In.
		2	45 "	I	*Sh.	Eg.
		3	44 "	I	*Tr.	Eg.
		9	39 P. M.	I	*Ec.	Dis.
	10	12	52 A. M.	I	*Oc.	Re.
		7	07 "	II	Sh.	In.
		9	03 "	II	Tr.	In.
		9	42 "	II	Sh.	Eg.
		11	40 "	II	Tr.	Eg.
		6	58 P. M.	I	Sh.	In.
		7	55 "	I	*Tr.	In.
		9	14 "	I	*Sh.	Eg.
		10	11 "	I	*Tr.	Eg.
	11	3	08 A. M.	III	*Ec.	Dis.
		5	42 "	III	Ec.	Re.
		6	52 "	III	Oc.	Dis.
Nov.	11	9	40 A. M.	III	Oc.	Re.
		3	48 P. M.	I	Ec.	Dis.
		7	18 "	I	*Oc.	Re.
	12	2	17 A. M.	II	*Ec.	Dis.
		6	44 "	II	Oc.	Re.
		1	26 P. M.	I	Sh.	In.
		2	22 "	I	Tr.	In.
		3	42 "	I	Sh.	Eg.
		4	38 "	I	Tr.	Eg.
	13	10	36 A. M.	I	Ec.	Dis.
		1	45 P. M.	I	Oc.	Re.
		8	25 "	II	*Sh.	In.
		10	13 "	II	*Tr.	In.
		10	59 "	II	*Sh.	Eg.
	14	12	50 A. M.	II	*Tr.	Eg.
		7	55 "	I	Sh.	In.
		8	48 "	I	Tr.	In.
		10	11 "	I	Sh.	Eg.
		11	04 "	I	Tr.	Eg.
		5	16 P. M.	III	Sh.	In.
		8	00 "	III	*Sh.	Eg.
		8	49 "	III	*Tr.	In.
		11	37 "	III	*Tr.	Eg.
	15	5	05 A. M.	I	*Ec.	Dis.
		8	12 "	I	Oc.	Re.
		3	35 P. M.	II	Ec.	Dis.
		7	54 "	II	*Oc.	Re.
	16	2	23 A. M.	I	*Sh.	In.
		3	15 "	I	*Tr.	In.
		4	39 "	I	*Sh.	Eg.
		5	31 "	I	*Tr.	Eg.
		11	33 P. M.	I	*Ec.	Dis.
	17	2	38 A. M.	I	*Oc.	Re.
		9	42 "	II	Sh.	In.
		11	23 "	II	Tr.	In.
		12	17 P. M.	II	Sh.	Eg.
		1	59 "	II	Tr.	Eg.
		8	52 "	I	*Sh.	In.
		9	41 "	I	*Tr.	In.
		11	08 "	I	*Sh.	Eg.
		11	58 "	I	*Tr.	Eg.
	18	7	06 A. M.	III	Ec.	Dis.
		9	42 "	III	Ec.	Re.
		10	19 "	III	Oc.	Dis.
		1	07 P. M.	III	Oc.	Re.
		6	02 "	I	Ec.	Dis.
		9	04 "	I	*Ec.	Re.
	19	4	52 A. M.	II	*Ec.	Dis.
		9	03 "	II	Oc.	Re.
		3	20 P. M.	I	Sh.	In.
		4	08 "	I	Tr.	In.
		5	36 "	I	Sh.	Eg.
		6	24 "	I	*Tr.	Eg.
	20	12	30 "	I	Ec.	Dis.
		3	31 "	I	Oc.	Re.
		11	00 "	II	*Sh.	In.
		12	32 "	II	*Tr.	In.
	21	1	35 A. M.	II	*Sh.	Eg.
		3	08 "	II	*Tr.	Eg.
		9	49 "	I	Sh.	In.
		10	34 "	I	Tr.	In.
		12	04 P. M.	I	Sh.	Eg.

	h	m					h	m			
Nov. 26	12	50	"	I	Tr. Eg.	Nov. 26	7	28	A. M.	II	Ec. Dis.
	9	15	"	III	*Sh. In.		11	20	"	II	Oc. Re.
	12	00	midn.	III	*Sh. Eg.		5	14	P. M.	I	Sh. In.
22	12	14	A. M.	III	*Tr. In.		5	52	"	I	*Tr. In.
	3	02	"	III	*Tr. Eg.		7	30	"	I	*Sh. Eg.
	6	59	"	I	Ec. Dis.		8	09	"	I	*Tr. Eg.
	9	57	"	I	Oc. Re.	27	2	24	P. M.	I	Ec. Dis.
	6	10	P. M.	II	*Ec. Dis.		5	16	"	I	Oc. Re.
	10	12	"	II	*Oc. Re.	28	1	35	A. M.	II	*Sh. In.
23	4	17	A. M.	I	*Sh. In.		2	49	"	II	*Tr. In.
	5	00	"	I	*Tr. In.		4	11	"	II	*Sh. Eg.
	6	33	"	I	Sh. Eg.		5	25	"	II	*Tr. Eg.
	7	16	"	I	Tr. Eg.		11	42	"	I	Sh. In.
24	1	27	A. M.	I	*Ec. Dis.		12	19	P. M.	I	Tr. In.
	4	23	"	I	*Oc. Re.		1	58	"	I	Sh. Eg.
	12	18	P. M.	II	Sh. In.		2	35	"	I	Tr. Eg.
	1	40	"	II	Tr. In.	29	1	13	A. M.	III	*Sh. In.
	2	53	"	II	Sh. Eg.		3	35	"	III	*Tr. In.
	4	18	"	II	Tr. Eg.		4	00	"	III	*Sh. Eg.
	10	46	"	I	*Sh. In.		6	23	"	III	Tr. Eg.
	11	26	"	I	*Tr. In.		8	53	"	I	Ec. Dis.
25	1	01	A. M.	I	*Sh. Eg.		11	42	"	I	Oc. Re.
	1	43	"	I	*Tr. Eg.		8	45	P. M.	II	*Ec. Dis.
	11	06	"	III	Ec. Dis.	30	12	28	A. M.	II	*Oc. Re.
	1	42	P. M.	III	Ec. Re.		6	11	"	I	Sh. In.
	1	43	"	III	Oc. Dis.		6	45	"	I	Tr. In.
	4	31	"	III	Oc. Re.		8	27	"	I	Sh. Eg.
	7	56	"	I	*Ec. Dis.		9	01	"	I	Tr. Eg.
	10	50	"	I	*Oc. Re.	Dec. 1	3	21	"	I	*Ec. Dis.
							6	08	"	I	Oc. Re.

NOTE.—In., denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc., denotes occultation; Tr., transit of the satellite; Sh., transit of the shadow; * Visible at Washington.

PRACTICAL SUGGESTIONS.

58. What proportion does the light that is reflected from the eight surfaces of the four lenses of a telescope sustain to the light that is transmitted to the eye?

W. E. S.

Answer: A thoroughly satisfactory answer to the question of W. E. S. is a very difficult task. The amount of light that is reflected from the eight surfaces of an achromatic system would vary according to the curves of the lenses themselves and it would be a very interesting problem for some one to take up photometrically, even this would be very difficult, because of the selective absorption of the various kinds of glass entering into the composition of an objective. However at a rough guess I should say that one-tenth of the light is lost in the reflections, at a minimum. That there is some light saved by cementing the inner surfaces there is no question, besides which it gets rid of ghosts that come into the field from those surfaces, but the cementing of an object glass larger than 3-inches in diameter has not been and cannot in the nature of the case be a successful operation. Many attempts at cementing large object glasses have been total failures. The element of irregular capillarity comes in as a function to distort the glass even when the cement is very soft, and when the cement hardens it is almost sure to pull irregullarly and destroy the beauty of the image at the focal plane. Even very thick lenses are affected in this way and it requires great care to cement them

so as to give a symmetrical image. I have examined lenses in which the cement had hardened, that, under the Foucault test, seemed to be twisted almost like a "cork screw" and although in reality this twisting was very small, it was enough to render the lenses of no value. J. A. B.

65. Should a lens be kept in a perfectly dry place? What material should be used in cleaning the object glass? A. K.

Answer: An object glass should not be kept in a damp place. The flint glass is subject to deterioration through years of exposure, particularly the softer flints. Oxygen, always existing in the atmosphere, unites with the metal in the glass to form a metallic oxide which is hastened by dampness. Therefore the safest way is to keep it in a dry place, and the right place to keep it is in the tube where it naturally belongs. Keep it covered up well when not in use. Even the hardest crown glass will slowly deteriorate from moisture, and while it takes a very long time to destroy the polish, one has only to examine the hard crown vases set in the Cesnola collection to see how glasses deteriorate under the action of moisture through long periods.

I think the so-called Japanese lens-paper is not suited for cleaning lenses. Follow the advice given in September POPULAR ASTRONOMY for cleaning lenses. I know of no better plan. J. A. B.

67. I would like to add to Mr. Brashear's directions for cleaning telescope objectives, a description of my own way of dealing with a dusty lens, with which annoyance I am very often troubled. It is simply to "swash" the lens with naphtha, holding it perpendicular meanwhile, until all the dust is washed down and off; the naphtha, if freely used, washes off every trace of grease or finger-marks at the same time, so that no wiping is necessary, and the lens dries off with a clean, "black-polished" surface, as if fresh from the maker's hands.

P. S. Y.

68. **An Illustration of the Principle of the Gegenschein.**—I have been much interested in the suggestions of Professor Barnard with regard to the cause of what is called the "Gegenschein." They have reminded me of a simple phenomenon which I have observed, and which seems to me to illustrate the same principle. If you walk out in a stubble field or a newly ploughed field, toward sunset or shortly after sunrise, as you look at your elongated shadow you will observe a very marked halo of light surrounding the shadow of your own head. The explanation of this very noticeable phenomenon is doubtless the same as that of the sudden and very marked increase of brightness of the Moon as it approaches the full. The line of vision of the beholder is so nearly the same as that of the Sun's rays, that a very large percentage of the individual points which make up the rough surface in question and are visible to the beholder, are squarely illuminated, while a very slight change of angle would bring into the line of sight numberless points which are in shadow, and which materially dilute the intensity of the reflected light. In the same way there is sometimes an intensely bright spot in the east at sunset. E. K.

So if we are surrounded by an approximately evenly distributed swarm of meteoric bodies, it would seem reasonable that the spot which is directly in opposition to the Sun should be the one in which their combined reflected light should be seen, if visible at all.

69. I would like to know how to obtain an electric light of about ½ candle power (at low cost) for reading circles, etc., in the Observatory. If you can suggest an arrangement of batteries that will furnish sufficient power for such a light, kindly do so. C.

Answer: An electric lamp of one candle power may easily be obtained by the use of two or more storage cells. Small commercial storage cells of 25 ampere hours capacity may be purchased.

In practice, it will be desirable to employ three storage cells, in series which may be charged by using seven gravity elements as the minimum number.

When only seven cells, however, are used to charge, about two ampere hours may be stored in one day. If the number of gravity elements is increased there will of course be an increased storage.

The gravity battery is connected to the storage cells in series and is left constantly in circuit. After the gravity battery has been set up, it is only necessary to add water and sulphate of copper occasionally; the zincs may be used three months or longer without cleaning. For regulating the amount of light, resistance coils may be introduced in the lamp circuit. H.

70. In Mr. Brashear's article on the care and cleaning of object-glasses, in the September number of your magazine—and I congratulate the readers of POPULAR ASTRONOMY upon the prospect of receiving instruction upon such themes from this accomplished artist,—he does me the honor to refer to certain directions in my *Amateur Telescopist's Handbook*, but dissents from my statement that the amateur should in no case separate or remove from their cell the two lenses of the object-glass, but leave it to an optician.

It is doubtless true that there has been a "halo" thrown around object-glasses which has caused the owners of them to refrain from any touching of them, except where absolute necessity compels. In the mind of Mr. Brashear, the object-glass is surrounded by no halo, for he handles objectives daily with the rarest skill. But I submit that it is much better and safer for the average amateur to have an exaggerated reverence for his object-glass than to have too little. The separation, cleaning and reconstruction of an objective is a matter of such ease to the trained optician that he naturally underrates the real difficulties and uncertainties of the operation; but it is only necessary to read Mr. Brashear's article to see that he is describing operations of a highly "ticklish" character for a novice.

When I wrote the sentence containing the warning above referred to, I had in mind the case of a superb 5-inch glass, the finest of its size that I ever saw, which was taken apart by its custodian, washed in soap and water, and replaced in the telescope. I happened to look at Venus through this glass a night or two afterwards, and was dismayed to find the planet surrounded by a flaming corona of "wings." Every effort to remedy the trouble failed, and the glass had eventually to be sent to an optician for rectification.

I fancy that after reading of the operations described by Mr. Brashear in the September number, and of the more delicate ones yet of reconstruction which he will describe in October, any careful student who loves his telescope will pause before undertaking them. And with all deference to the distinguished artist who is instructing us, I must still continue to repeat the advice which I gave my fellow-amateurs in my *Handbook*; if it is necessary to take the object-glass apart, let it be done by the maker, or by a competent optician—say Mr. Brashear.

FRANK M. GIBSON.

Washington, D. C., Sept. 7, 1894.

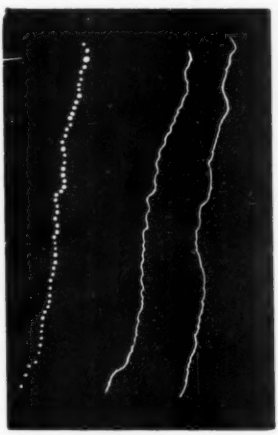
71. In the May number Dr. Veeder states that "it is the position and not the size of the spots that determines the auroral effect." It is proper to note that MM. Terby and Marchand have proposed a central theory, while Dr. Veeder proposes an eastern limb theory.

The great spot group of '92 and the corresponding magnetic perturbations and auroral display did not conform to Dr. Veeder's views. It has also been shown recently that no serious magnetic "storm" accompanied the celebrated observation of Hodgson and Carrington in '59. About two years ago Lord Kelvin said "The magnetic storm of June 25, '85, showed that in eight hours of a not very severe magnetic storm as much work must have been done by the Sun in sending magnetic waves out in all directions through space as he does in four months of his regular heat and light. This result, it seemed to him, was absolutely conclusive against the supposition that terrestrial magnetic storms were due to magnetic action of the Sun or to any kind of dynamical action taking place within the Sun or in connection with hurricanes in his atmosphere or anywhere near the Sun outside."

That Dr. Veeder's commendable observations in connection with the highly valuable plates of the solar surface as now obtained by Deslandres and Hale may in the future shed some light upon the matter seems quite probable; the eastern limb theory as yet, however, seems not conclusively established nor has it obtained general recognition. A. C. BEHR.

1332 Lexington Ave., Chicago.

72. Having given some attention to photographing lightning and studying the records made upon the sensitive plate, I have been more observant than ever.



To-day while driving over the country midst a terrific thunderstorm, I observed many fine flashes, one instance of which deserves particular mention. Two substantially parallel simultaneous flashes, somewhat wavy, seemed to pass toward the Earth. Length about 40°, distance apart about 7°, both brilliant, but not blindingly so. Both, as lines of light, vanished instantly, but the one to the right broke into a multiplicity of what seemed to be spheres. This string of beads, as it seemed, appeared to be formed of material furnished by the original flash.

In the accompanying sketch the two full lines represent the original flashes, while the line of dots represents the after effect of the flash nearer it. The phenomenon was observed by Mr. C. A. Rand who was riding with me. That there was no optical illusion is evidenced by the fact that only one of the flashes made the showing referred to.

Denver, Col. J. F. STEWARD.

73. In the article on "Folk-love Astronomy" in the September number of POPULAR ASTRONOMY, there is an explanation of the faint reflections or "satellites" seen when a planet is looked at in a common mirror, which, it may be worth while pointing out, is erroneous. If the front and back surfaces of the mirror are parallel, as shown in the figure, the virtual images formed by them will be infinitesimally displaced at a (practically) infinite distance, and hence will coincide; or, what is the same thing, all the reflected rays reaching the eye will be parallel, and therefore form a single image on the retina. In order to form images sensibly displaced, the reflecting surfaces must be inclined at a small angle; *i. e.*, the mirror must be a bad one.

An amusing incident relating to this subject was told to the writer some years ago, by a lady then living near the sea-coast. After reading in a newspaper paragraph that the satellites of Mars could be seen in an ordinary looking-glass, she resolved to repeat the experiment, and arming herself with a small hand-mirror, went out into the open air with a number of friends. Mars was shining brightly low in the east, and all the party were much pleased to find that the satellites were easily visible. After they had spent considerable time in these observations, some one remarked that the planet did not seem to be getting any higher, and on closer investigation it was found that they had been observing the distant light-house! K.

74. The Chicrgo *Tribune* of Aug. 4, published a short article under the title, "Think Marsians want to talk." It is claimed that observers in France have seen bright points on the surface of Mars which they construe as light signals for the observers on the Earth. In other words, the people of Mars wish to talk with the people of the Earth and so are trying to get our attention in this way.

Of course all such talk is pure fancy and nothing more. There is no evidence of any artificial light seen by anybody on the surface of Mars that we ***know*** of.

75. "W. E. S.," in Practical Suggestions, No. 55, evidently misunderstands the question asked by "W. B. H.," in Suggestion No. 24. "W. B. H.," asks why one should not use a lower power than 160 in viewing stars like the companion of Polaris. He says that with 160 on 3½-inch he fails to see it; but I judge that with a lower power and with same aperture he *can* see it. This has been in some degree my own experience. Frequently with 120 on 3-inch it is difficult; with 80 less difficult; and with 64 easy. I have seen it with only 16 on 3-inch telescope. Other stars which Capt. Noble speaks of as requiring high power are better seen with a low one; at least it is so in my case. 11 Aquilæ, mentioned as "severe test for 3-inch glass," is usually easy with 64, but difficult with 120 or more. Select the power best suited to your own eye, seems to be the right rule to go by.

Q. A. W.

76. I was greatly interested in G. P. Serviss' account of the performance of his short-focus 5-inch refractor. I have a 5-inch of nearly the same focal length viz., 52 inches instead of 52½. This glass I ground, polished and corrected myself. I wanted to see what could be done with so short a focal length and am well satisfied with the results. Some authorities have said that an objective of long focus gives better definition than one of short focal length, and this is undoubtedly true where neither glass has been treated for spherical aberration, but where both glasses are perfectly corrected for this fault,—which in case of the short focus glass incurs much more work—the definition should be if anything in favor of the shorter focus. In testing this point it should be remembered that where the same eye-piece is used for both glasses, it gives a higher magnification with the longer focus and a consequent better definition and diminished illumination. There is however one point against the short-focus though I think it so slight in small glasses that it has practically no weight against their advantages. That is the better color correction of the long-focus objective. If the lens be tilted slightly to one side so that its axis does not coincide with that of the tube and the eye-piece, or if the centres of the flint and crown discs be not concentric greater distortion and color error than in a larger telescope of the same aperture are caused.

F. F. A.

77. Some time ago the question was asked why the central two inches of a certain objective should perform better than a larger area out side of that two

inches. The answer given, was true as far as it went. More possibly might have been said. If a first-class objective would not show such a difference between the performance of the centre and edge, why should this objective? I would say that the lens in question might have had any or all of the following faults: It might have been only partially corrected for spherical aberration. It might have local errors in the edge that it did not have in centre though this is improbable. It might not have been perpendicular to the axis, the tube and eye-piece. The two lenses may not have been perfectly centred. The edge of the lens may not have been as perfectly polished as the centre. Any of these things would cause much more serious disturbance at the edge of objective than in the centre and in case the color correction was the best that could be done, the secondary spectrum would be more marked in rays which passed through the outer portions than in those which passed through the central part. F. F. A.

73. From 2:45 this morning till dawn there was the most brilliant display of aurora I ever saw. I have seen auroras with more variety of color—the prevailing tints in this were white and pink. The Moon being only three or four days past the full was very bright, but notwithstanding this fact I could sometimes trace the streaks of light to within 2° of that orb. At times the whole heavens seemed illuminated with the streaks, the only place where I could not distinctly see them being in the immediate neighborhood of the Moon and in a strip extending southward from the Moon and about 30° wide at the horizon and 4° to 5° wide at the Moon. The streaks converged at a point about 10° south of the zenith, and this point did not change position perceptibly during my observations. F. F. A.

ROCKFORD, Ill., Aug. 20, 1894.

GENERAL NOTES.

This issue is unexpectedly delayed ten days on account of engraving work that could not be safely hurried. We will try to be on time for November.

We have not felt at liberty to send this number to subscribers who have not renewed for Volume II, because we have thought they do not wish us to do so.

Colored Map of Mars.—Professor J. M. Schaeberle of Lick Observatory furnished us excellent copy of his recent work at Lick Observatory on Mars, in the form of a finely colored plate which has been reproduced for October *Astronomy and Astro-Physics*. We also present it to the readers of this publication.

Reorganization of the U. S. Naval Observatory.—On Sept. 21, H. A. Herbert, Secretary of the Navy, wrote to Professor William Harkness, saying substantially that he had appointed him Astronomical Director of the Naval Observatory, to be in charge of, and responsible for the direction, scope, quantity and preparation for publication of all work purely astronomical to be performed at the Observatory. The Secretary admits that the criticisms of astronomers in regard to the lack of system in astronomical work at the Observatory has some foundation, but that all other charges are groundless.

The new regulations under which this appointment is made provide that the Observatory, under the control of the Secretary of the Navy, is subject to the

direct supervision of the bureau of equipment, that a naval officer is assigned as superintendent and that the Observatory work is divided in two branches, astronomical and nautical. The first of these includes the department of astronomical observations, and the department of the nautical almanac. The second includes the departments of nautical instruments, of chronometers, and time service and of magnetism and meteorology. The superintendent, as commanding officer, is charged with the general superintendence and government of the Observatory.

Duties of the Astronomical Director.—All officers, assistant astronomers, computers and employes are subject to his orders and he reports the operations of the Observatory annually. The astronomical director has charge of and is responsible for the direction, scope, character, quantity and preparation for publication of all work purely astronomical which is performed at the Observatory. He has charge of the 26-inch and 12-inch equatorial telescopes, the 6-inch and 9-inch transit circles and in fact all the astronomical instruments. He is to personally inspect, both day and night the methods of observation and is to present to the department on the last days of June and December of each year reports upon the qualities of his subordinates, their aptitude, efficiency, zeal, punctuality, health, and deportment.

He is not to enter into any agreement with other Observatories for the performance of work which will occupy the time of any observer more than one month without the sanction of the department.

The director of the Nautical Almanac is held directly responsible for all that pertains to that publication.

Nautical Instruments and Time.—The head of the department of nautical instruments is to see that all the instruments issued except chronometers are thoroughly inspected, that a record shall be kept of each instrument, and is held responsible for the safe keeping of instruments.

The head of the chronometer and time service department is to make all the determinations of local time that are necessary, will transmit the daily time signals and care for all the time and chronometer work of the Observatory. The head of the magnetism and meteorology department is to keep up a continuous series of observations and report any unusual disturbances, their probable cause and relation to visible phenomena.

An annual report of the operations in the various departments for the preceding fiscal year should be submitted to the superintendent July 15.

The regulations for the supervision of the library, the buildings and grounds, those governing the instrument maker, electrician, watchmen, fire organization and laborers are all placed under the control of the superintendent.

The Latest about Mars.—The *Martha's Vineyard Herald* of August 25 has the laugh on us astronomers in pretty good style. See what it says:

Marseania has struck the fellows who are running the big Lick telescope. They have it bad.

According to them, Mars has no atmosphere at all, and consequently no life exists on that planet. This is symptom No. 1.

Mars has no more atmosphere than the Moon. It is a dead world, of no more value than the state of Nevada which only supports two Republican senators. Symptom No. 2.

If Mars has an atmosphere it is not one-quarter as extensive as that of the Earth. Symptom No. 3.

No. 4 is, the atmosphere of Mars is so thin a man from the Earth would be discouraged if he tried to breathe it for a steady occupation.

Well then, who dug 'them' canals on Mars?

Who set up 'them' signals on Mars and lighted 'em'?

The Transit of Mercury Nov. 10, 1894.—The most important astronomical event predicted for this year is the transit of Mercury across the Sun's disc which will take place Nov. 10. This phenomenon occurs only about once in seven years, on the average, so that not many opportunities to observe it come in a life time. The amateur as well as the professional astronomer will be expected to make the most of this opportunity.

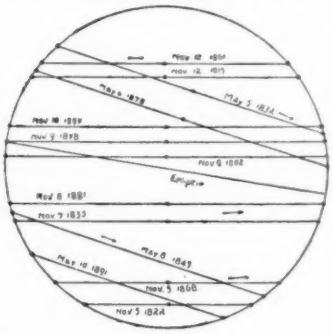


Diagram showing the Paths of Mercury across the Sun's Disc during the Transits of this Century.

This transit will be visible in Western Europe and Africa, the Atlantic Ocean, North and South America, and the Pacific Ocean. The Sun will be most favorably situated for observation in Central and South America, but the conditions will also be good in the United States. The planet will enter upon the disc of the Sun near the East point, 98° around the limb from the North point, at $9^h\ 55^m\ 18^s$ A. M., Central Standard time, as seen from Northfield, and will take a northwesterly course, leaving the disc at a point 50° west from the north point, at $3^h\ 12^m\ 15^s$ P. M.

The planet will appear as a round *very black* spot, distinguishable from ordinary sunspots by its color, its roundness and its motion. The observer should watch carefully to see if, as it enters upon and leaves the disc of the Sun the planet is encircled by a ring of light, and if, when fully on the disc, it is surrounded by a narrow dusky fringe. These if seen would be evidences of an extensive atmosphere upon the planet.

ELEMENTS OF THE TRANSIT.

Greenwich mean time of conjunction in R. A. Nov. 10, $6^h\ 54^m\ 16^s.3$.

Sun and Mercury's R. A.	$15^h\ 03^m\ 44^s.68$	Hourly motion	$+10^s.12$ and $-12^s.46$
Sun's declination	17° 18′ 58″.2S.	Hourly motion	0′ 41″.8S.
Mercury's declination	17 14 05 .2S.	Hourly motion	1 45 .2N.
Sun's equa. hor. par.	8 .94	True semidiameter	16 09 .83
Mercury's equa. hor. par.	13 .08	True semidiameter	4 .94

GREENWICH MEAN TIMES OF THE PHASES.

Ingress, exterior contact	November 10,	3^h	55^m	$31^s.2$
Ingress, interior contact		3	57	15 .4
Least distance of centres, 4′ 26″.8		6	33	48 .5
Egress, interior contact		9	10	26 .4
Egress, exterior contact		9	12	10 .4

The Greenwich mean time of exterior contacts, for any point on the Earth's sutface, may be computed from the following formulæ, in which ρ denotes the radius of the Earth at that place, φ' the geocentric north latitude and λ the longitude west from Greenwich.

Ingress $T_1 = 3^h\ 55^m\ 31^s.2 + [0.7793]^s\ \rho \sin \varphi'$
$\quad - [1.6352]^s\ \rho \cos \varphi' \cos (329° 28′ 15″ - \lambda)$

Egress $T_2 = 9^h\ 12^m\ 10^s.4 + [1.4333] \rho \sin \varphi'$
$\quad + [1.5339]^s\ \rho \cos \varphi' \cos (218° 51′ 07″ - \lambda)$

In the following table we give the Greenwich times of beginning and ending of the transit, calculated for the several Observatories from the American Nautical Almanac data:

Observatory.	Longitude.		Latitude.		Transit begins.				Transit ends.			
	°	′	°	′	h	m	s		h	m	s	
Harvard	71	07.7	+42	23	3	55	29	P. M.	9	12	07	P. M.
Washington	77	03.0	+38	54	3	55	24	"	9	12	06	"
Goodsell	93	09.0	+44	28	3	55	18	"	9	12	15	"
Chamberlin	104	51.9	+39	41	3	55	11	"	9	12	17	"
Lick	121	38.5	+37	20	3	55	04	"	9	12	23	"

It will be seen that the times vary only in the seconds throughout the whole United States. In order to get the Standard times we have only to subtract 5 hours for eastern, 6 hours for central, 7 hours for mountain and 8 hours for Pacific time.

Mr. Thos. Lindsay, assistant secretary of the Astronomical and Physical Society of Toronto sends us the following times of the contacts at the beginning of the transit at Northfield.

Exterior contact $9^h\ 55^m\ 50^s.0$ A. M.
Interior contact $9^h\ 57^m\ 28^s.4$ " } Central time

These he computed from the data given in the British Nautical Almanac, considering the transit as an occultation of the Sun by Mercury.

The difference between his result and that given above is chiefly due to the difference in time of conjunction in right ascension as given in the two almanacs. The American gives the Greenwich time of conjunction as $6^h\ 54^m\ 16^s.3$ while the British almanac gives it as $6^h\ 54^m\ 47^s.6$.

Cause of the Gegenschein.—I have read Dr. Barnard's article on the Gegenschein in POPULAR ASTRONOMY for April with considerable interest.

The suggestion that it is due to an enormous number of small asteroids appears the most probable one, but it seems to me there are some serious difficulties in the way. Supposing these to be of small, but not infinitesimal, dimensions, and in such numbers as to cause a continuous glow, ought not the stars situated along the Zodiacal belt to be subject to frequent momentary occultations?

Granting that they are, and that these momentary occultations cannot be distinguished from ordinary twinkling, a point I feel by no means sure about, ought not the Zodiacal stars to twinkle more than those remote from the ecliptic? Has this been observed?

Again, if these small asteroids have only infinitesimal dimensions, to cause a continuous glow, they must be in almost infinite numbers, and would cause the asteroidal belt to be occupied by a resisting medium, and in that case the fact could not fail to be evident ultimately by the motions of the asteroids, or immediately by the effect on any comet which happened to cross the belt. Has anything of the sort been noticed?

Is it impossible that both the Gegenschein and the Zodiacal light may be due to a ring of very small particles surrounding the Earth, but inside the Moon's orbit? These may be due either to a former satellite, or to matter ejected from the former volcanoes of the Moon. If Mr. Coakley's theory of the origin of meteorites (*Astronomy and Astro-Physics*, p. 753, vol. XI) be true, there must be an enormous number of these bodies within the limits of the lunar orbit, and they must cause phenomena analogous to the Gegenschein and Zodiacal light: in fact, if these phenomena did not exist, I would regard their non-existence as to some extent evidence against Mr. Coakley's theory.

Of course the objections to the asteroidal hypothesis may also be urged against this suggestion, but it seems to me with much lessened force. In general I take it that these bodies are of very small dimensions, mere cosmic dust, and in that case they would not cause occultations or twinkling of the fixed stars; their

effect would merely be to cause a general and nearly constant dimming of the light, which could not be detected. Of course, if any comet happened to cross this belt, the same effect would be produced as on the former hypothesis, but owing to its very much smaller size, the chance of a comet crossing it would be infinitesimal.

Would there be any chance of seeing any part of such a ring during a total solar eclipse? J. R. HOLT.

6 Harrington Street, Dublin, Ireland.

Variable Stars. Mr. W E. Sperra, of Randolph Ohio, has sent me the following maxima and minima, from observations made with a 3-inch refractor:

4805. W Virginis.

A maximum is indicated for 1894, June 21, and a minimum quite definitely established for June 30. Between May 26 and June 8 there are 15 observations.

5194. V Böotis.

Mr. Sperra's observations give a maximum 1894, June 12. My own observations, 21 in number, give a minimum, rather uncertainly, 1894 March 21, $9^m.2$ and a maximum, June 19, $7^m.0$. The star changes very slowly near its maximum so that the exact date is difficult to determine, and the accordance is as close as could be expected.

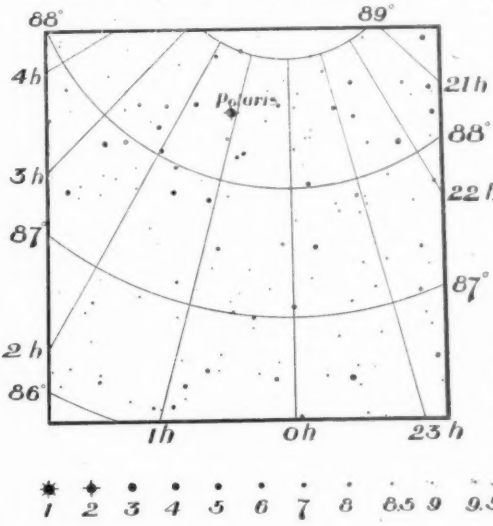
7085. R T Cygni.

Mr. Sperra locates a maximum 1894 June 20. My observations, 25 in number, yield a maximum June 17, $7^m.1$.

In addition to these I would report a maximum and a minimum of 8324 V Cassiopeiæ, the variable discovered by Anderson of Edinburgh in 1893. I have 40 observations between 1894, January 18 and August 20. A minimum of the 12th magnitude occured 1894, March 21, and a maximum of the 7.2 magnitude 1894. July 15. The light curve is smooth and the change rapid. J. A. PARKHURST.

Marengo, Ill., 1894 September 19.

In answer to queries kindly submitted I would say:



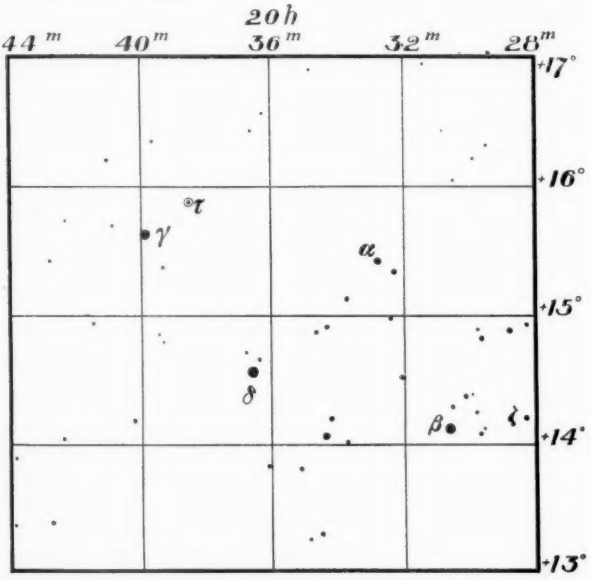
1. The star known as the "companion" of Polaris is situated about 18″ from its principal, at a position-angle of 210°. It is a faint 9th magnitude star, and is so close to the large star as never to be a conspicuous object with apertures up to 4 inches.

Dawes, whose keenness of eye is famous, puts the least optical aid with which it can possibly be distinguished, as a power of 80 on a 2-inch refractor. On the 10th of the present month, in very good seeing I found it barely distinguishable with 30 on my 4¼-inch glass.

It seems needless to add that no field-glass has the remotest chance of showing it.

The accompanying chart, from the Durchmusterung, shows the small stars in the neighborhood of Polaris.

2. The above chart, also from the Dutchmusterung, shows the small stars of Delphinus to the 8th magnitude.



The 6th magnitude star just preceding α Delphini, is a Durchmusterung star, and is also listed in the British Association Catalogue; the former authority gives its magnitude as 6.0, and the latter as 7.0. It has never to my knowledge been suspected of variability.

It is idle to suspect variation in any star on account of its absence from any naked-eye map on which other stars of the same nominal magnitude are given, or on account of discordances in the magnitudes assigned by different authorities or in different catalogues.

3. In all observations of variable or suspected variable stars, the utmost care should be taken to make certain of the identification of the object.

P. S. Y.

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GALLE (J. G.) Verzeichnss der Elemente der leisher berechneten Cometenbahnen, nebst Anmerkungen und Literatur—Nachweisen, fortgesetzt bis 1894. 8vo. Leipzig, 1894. $3.13.

HIGGS (G.) Photographic atlas of the normal solar spectrum, 106 maps, royal 8vo, in cloth case, 1894, original size, 15 parts. $5.25, two diameters; 45 parts, $15.75. Four diameters, 44 parts, $42.

KIEBEL (A.) Galilei's Untersuchung der Fallbewegung, with engravings, 8vo. Zernowitz 1894. 25 cents.

ROBERTS (S.) A selection of photographs of stars, star-clusters and nebulæ, together with information concerning the instruments and the methods employed in the pursuits of celestial photography, 53 plates. 4to. cloth. 1894. $7.50.

SPOERER (G.) Beobachtungen von Sonnenfleken with 30 plates. 4to. Leipzig, 1894. $3.13.

PUBLISHERS' NOTICES.

This magazine now has on its lists subscribers in thirty-five States of the Union, as well as in many foreign countries. Probably the oldest subscriber is Mr. Francis Coleman, of Hamilton, Ontario, Canada, who writes: "Although soon to enter my eighty-second year, I will try your instructive periodical on my favorite science for another six months."

Advertising space in POPULAR ASTRONOMY can be secured by application to the publisher.

We ask the favor of payment at an early date from those who renew their subscriptions at the beginning of Vol. II.

The celestial photographs made at Goodsell Observatory are being very favorably received. The last mail brings a large order from Newburgh, New York. These photographs may be ordered as mounted or unmounted prints, or as lantern slides. For list of subjects see advertisement in this issue.

We extend our thanks to the many friends who have sent us names of new subscribers, or the addresses of persons interested in astronomy but not on our subscription books. Such aid is of the greatest value to us.

When a subscription to POPULAR ASTRONOMY expires the subscriber's name is retained on the books for one month; after that time, if no notice of renewal is received, the name is dropped from the lists. Promptness in sending orders for renewal or discontinuance is greatly desired.

As at this season astronomy classes are beginning their work in the schools, it is suggested that many may be interested in the club rates offered by POPULAR ASTRONOMY. We invite correspondence on this subject. The magazine serves as a supplement to the text book in that it presents the sky as it is *to-day;* states when and where the planets may be seen; calls attention to expected astronomical events of interest; gives the latest news of astronomical discoveries, and helpful suggestions as to observations; its illustrations make clear much which words seem to obscure. It records in untechnical language the work and the conclusions of the leading astronomers of our own day.

Attention is called to the advertisement of the Minneapolis & St. Louis Railway, found at the close of this number. The tourist car accommodations here offered will be of interest to any desiring to combine small expense with comfort in traveling.

MAP OF THE CONSTELLATIONS

VISIBLE OCTOBER 15, .. AT 9 P. M.

REDUCED FROM SECTION OF POOLE BROTHERS' PLANISPHERE.

BY PERMISSION.

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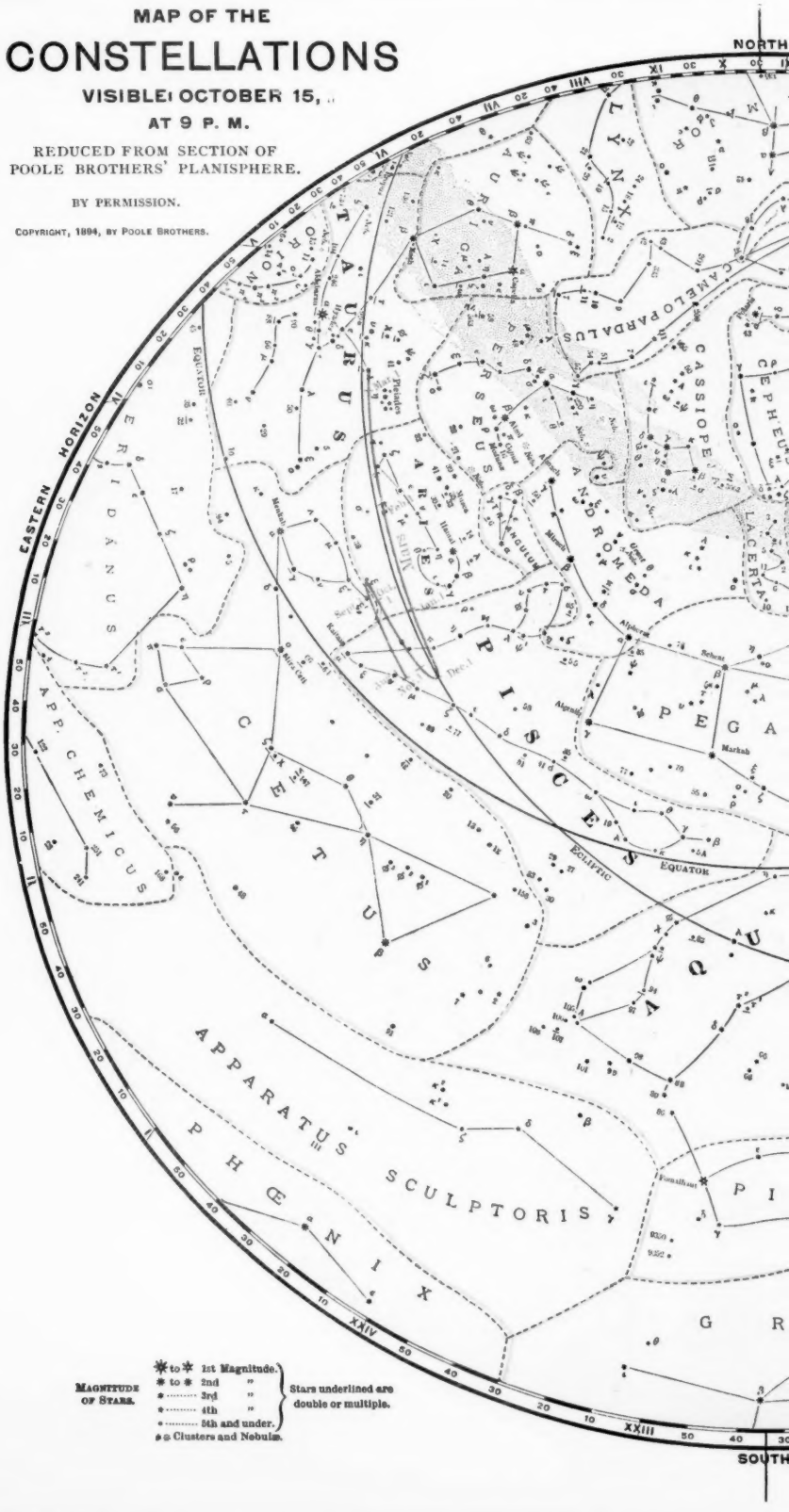
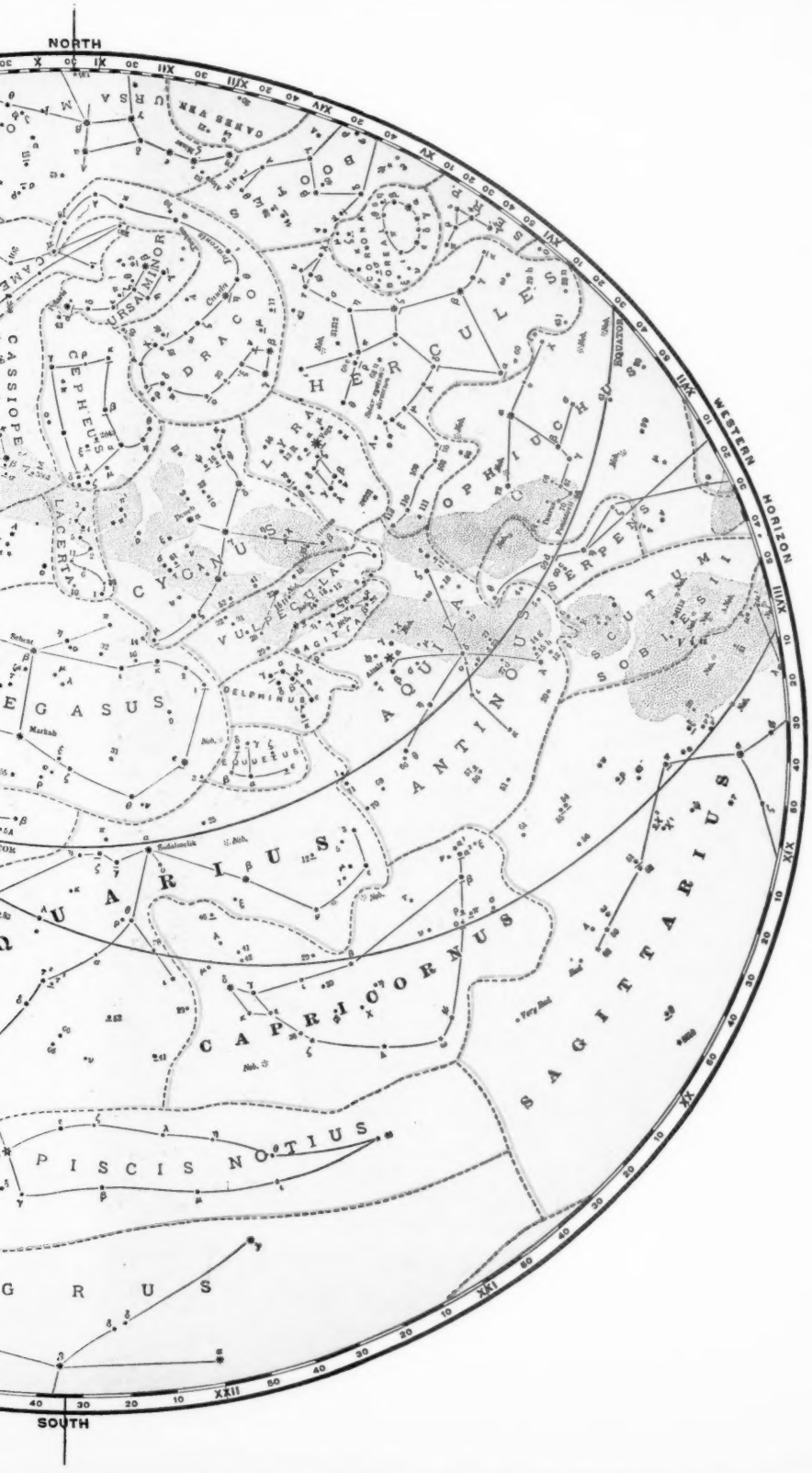


MAGNITUDE OF STARS.		
✱ to ✱	1st Magnitude.	Stars underlined are double or multiple.
✱ to ✱	2nd "	
✱	3rd "	
✱	4th "	
•	5th and under.	
⊕ ⊛	Clusters and Nebulæ.	


NORTH
SOUTH
WESTERN HORIZON
URSA MAJOR
URSA MINOR
DRACO
CEPHEUS
CASSIOPEIA
CAMELOPARDALIS
BOOTES
HERCULES
LYRA
CYGNUS
LACERTA
VULPECULA
SAGITTA
DELPHINUS
EQUULEUS
OPHIUCHUS
SERPENS
AQUILA
ANTINOUS
SCUTUM
SOBIESKI
PEGASUS
AQUARIUS
CAPRICORNUS
SAGITTARIUS
PISCIS NOTIUS
GRUS
Markab
Scheat
Enif
Deneb Adige
Altair
Rasalhague
Antares
Fomalhaut
Sadalmelik
Vega
Arcturus
Alphecca
Very Red